PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCVIII.

PART I.

LONDON,

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FOR THE YEAR MDCCCVIII, being the
Second of their New Series.

PART I.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

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be, the importance and singularity of the subjects, or the advantageous manner of treating them, without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the Year 1807, to EVERARD HOME, Esq. F. R. S. for his various Papers on Anatomy and Physiology printed in the Philosophical Transactions.

PHILOSOPHICAL TRANSACTIONS.

I. *The Bakerian Lecture, on some new Phenomena of chemical Changes produced by Electricity, particularly the Decomposition of the fixed Alkalies, and the Exhibition of the new substances which constitute their bases; and on the general Nature of alkaline Bodies.* By Humphry Davy, *Esq. Sec. R.S. M. R. I. A.*

Read November 19, 1807.

I. *Introduction.*

In the Bakerian Lecture which I had the honour of presenting to the Royal Society last year, I described a number of decompositions and chemical changes produced in substances of known composition by electricity, and I ventured to conclude from the general principles on which the phenomena were capable of being explained, that the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies.

This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts. In the course of a laborious experimental application of the powers of electro-chemical analysis, to bodies which have appeared simple when examined by common chemical agents, or which at least have never been decomposed, it has been my good fortune to obtain new and singular results.

Such of the series of experiments as are in a tolerably mature state, and capable of being arranged in a connected order, I shall detail in the following sections, particularly those which demonstrate the decomposition and composition of the fixed alkalies, and the production of the new and extraordinary bodies which constitute their bases.

In speaking of novel methods of investigation, I shall not fear to be minute. When the common means of chemical research have been employed, I shall mention only results. A historical detail of the progress of the investigation, of all the difficulties that occurred, and of the manner in which they were overcome, and of all the manipulations employed, would far exceed the limits assigned to this Lecture. It is proper to state, however, that when general facts are mentioned, they are such only as have been deduced from processes carefully performed and often repeated.

II. *On the Methods used for the Decomposition of the fixed Alkalies.*

The researches I had made on the decomposition of acids, and of alkaline and earthy neutral compounds, proved that the powers of electrical decomposition were proportional to the strength of the opposite electricities in the circuit, and

to the conducting power and degree of concentration of the materials employed.

In the first attempts, that I made on the decomposition of the fixed alkalies, I acted upon aqueous solutions of potash and soda, saturated at common temperatures, by the highest electrical power I could command, and which was produced by a combination of VOLTAIC batteries belonging to the Royal Institution, containing 24 plates of copper and zinc of 12 inches square, 100 plates of 6 inches, and 150 of 4 inches square, charged with solutions of alum and nitrous acid; but in these cases, though there was a high intensity of action, the water of the solutions alone was affected, and hydrogene and oxygene disengaged with the production of much heat and violent effervescence.

The presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. By means of a stream of oxygene gas from a gasometer applied to the flame of a spirit lamp, which was thrown on a platina spoon containing potash, this alkali was kept for some minutes in a strong red heat, and in a state of perfect fluidity. The spoon was preserved in communication with the positive side of the battery of the power of 100 of 6 inches, highly charged; and the connection from the negative side was made by a platina wire.

By this arrangement some brilliant phenomena were produced. The potash appeared a conductor in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the developement of combustible matter, arose from the point of contact.

When the order was changed, so that the platina spoon

was made negative, a vivid and constant light appeared at the opposite point: there was no effect of inflammation round it; but aeriform globules, which inflamed in the atmosphere, rose through the potash.

The platina, as might have been expected, was considerably acted upon; and in the cases when it had been negative, in the highest degree.

The alkali was apparently dry in this experiment; and it seemed probable that the inflammable matter arose from its decomposition. The residual potash was unaltered; it contained indeed a number of dark grey metallic particles, but these proved to be derived from the platina.

I tried several experiments on the electrization of potash rendered fluid by heat, with the hopes of being able to collect the combustible matter, but without success; and I only attained my object, by employing electricity as the common agent for fusion and decomposition.

Though potash, perfectly dried by ignition, is a nonconductor, yet it is rendered a conductor, by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers.

A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery of the power of 250 of 6 and 4, in a state of intense activity; and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere.

Under these circumstances, a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces.

These globules, numerous experiments soon shewed to be the substance I was in search of, and a peculiar inflammable principle the basis of potash. I found that the platina was in no way connected with the result, except as the medium for exhibiting the electrical powers of decomposition; and a substance of the same kind was produced when pieces of copper, silver, gold, plumbago, or even charcoal were employed for compleating the circuit.

The phenomenon was independent of the presence of air; I found that it took place when the alkali was in the vacuum of an exhausted receiver.

The substance was likewise produced from potash fused by means of a lamp, in glass tubes confined by mercury, and furnished with hermetically inserted platina wires by which the electrical action was transmitted. But this operation could not be carried on for any considerable time; the glass was rapidly dissolved by the action of the alkali, and this substance soon penetrated through the body of the tube.

Soda, when acted upon in the same manner as potash, exhibited an analogous result; but the decomposition demanded

greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces. With the battery of 100 of 6 inches in full activity I obtained good results from pieces of potash weighing from 40 to 70 grains, and of a thickness which made the distance of the electrified metallic surfaces nearly a quarter of an inch; but with a similar power it was impossible to produce the effects of decomposition on pieces of soda of more than 15 or 20 grains in weight, and that only when the distance between the wires was about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch.

The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared having the lustre of silver.

When the power of 250 was used, with a very high charge for the decomposition of soda, the globules often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air in a state of vivid combustion, producing a beautiful effect of continued jets of fire.

III. *Theory of the Decomposition of the fixed Alkalies; their Composition, and Production.*

As in all decompositions of compound substances which I had previously examined, at the same time that combustible bases were developed at the negative surface in the electrical circuit, oxygene was produced, and evolved or carried into combination at the positive surface, it was reasonable to con-

clude that this substance was generated in a similar manner by the electrical action upon the alkalies; and a number of experiments made above mercury, with the apparatus for excluding external air, proved that this was the case.

When solid potash, or soda in its conducting state, was included in glass tubes furnished with electrified platina wires, the new substances were generated at the negative surfaces; the gas given out at the other surface proved by the most delicate examination to be pure oxygene; and unless an excess of water was present, no gas was evolved from the negative surface.

In the synthetical experiments, a perfect coincidence likewise will be found.

I mentioned that the metallic lustre of the substance from potash immediately became destroyed in the atmosphere, and that a white crust formed upon it. This crust I soon found to be pure potash, which immediately deliquesced, and new quantities were formed, which in their turn attracted moisture from the atmosphere till the whole globule disappeared, and assumed the form of a saturated solution of potash.*

When globules were placed in appropriate tubes containing common air or oxygene gas confined by mercury, an absorption of oxygene took place; a crust of alkali instantly formed upon the globule; but from the want of moisture for its

* Water likewise is decomposed in the process. We shall hereafter see that the bases of the fixed alkalies act upon this substance with greater energy than any other known bodies. The minute theory of the oxydation of the bases of the alkalies in the free air, is this:—oxygene gas is first attracted by them, and alkali formed. This alkali speedily absorbs water. This water is again decomposed. Hence, during the conversion of a globule into alkaline solution, there is a constant and rapid disengagement of small quantities of gas.

solution, the process stopped, the interior being defended from the action of the gas.

With the substance from soda, the appearances and effects were analogous.

When the substances were strongly heated, confined in given portions of oxygene, a rapid combustion, with a brilliant white flame was produced, and the metallic globules were found converted into a white and solid mass, which in the case of the substance from potash was found to be potash, and in the case of that from soda, soda.

Oxygene gas was absorbed in this operation, and nothing emitted which affected the purity of the residual air.

The alkalies produced were apparently dry, or at least contained no more moisture than might well be conceived to exist in the oxygene gas absorbed; and their weights considerably exceeded those of the combustible matters consumed.

The processes on which these conclusions are founded will be fully described hereafter, when the minute details which are necessary will be explained, and the proportions of oxygene, and of the respective inflammable substances which enter into union to form the fixed alkalies, will be given.

It appears then, that in these facts there is the same evidence for the decomposition of potash and soda into oxygene and two peculiar substances, as there is for the decomposition of sulphuric and phosphoric acids and the metallic oxides into oxygene and their respective combustible bases.

In the analytical experiments, no substances capable of decomposition are present but the alkalies and a minute

portion of moisture; which seems in no other way essential to the result, than in rendering them conductors at the surface: for the new substances are not generated till the interior, which is dry, begins to be fused; they explode when in rising through the fused alkali they come in contact with the heated moistened surface; they cannot be produced from crystallized alkalies, which contain much water; and the effect produced by the electrization of ignited potash, which contains no sensible quantity of water, confirms the opinion of their formation independently of the presence of this substance.

The combustible bases of the fixed alkalies seem to be repelled as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces, and the oxygene follows the contrary order;* or the oxygene being naturally possessed of the negative energy, and the bases of the positive, do not remain in combination when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected; but when they are exalted by heat, a rapid union is the result; and as in other like cases with the production of fire.—A number of circumstances relating to the agencies of the bases of the alkalies will be immediately stated, and will be found to offer confirmations of these general conclusions.

* See Bakerian Lecture 1806, page 28 Phil. Trans. for 1807.

IV. *On the Properties and Nature of the Basis of Potash.*

After I had detected the bases of the fixed alkalies, I had considerable difficulty to preserve and confine them so as to examine their properties, and submit them to experiments; for, like the *alkahests* imagined by the alchemists, they acted more or less upon almost every body to which they were exposed.

The fluid substance amongst all those I have tried, on which I find they have least effect, is recently distilled naphtha.—In this material, when excluded from the air, they remain for many days without considerably changing, and their physical properties may be easily examined in the atmosphere when they are covered by a thin film of it.

The basis of potash at 60° FAHRENHEIT, the temperature in which I first examined it, appeared, as I have already mentioned, in small globules possessing the metallic lustre, opacity, and general appearance of mercury; so that when a globule of mercury was placed near a globule of the peculiar substance, it was not possible to detect a difference by the eye.

At 60° FAHRENHEIT it is however only imperfectly fluid, for it does not readily run into a globule when its shape is altered; at 70° it becomes more fluid; and at 100° its fluidity is perfect, so that different globules may be easily made to run into one. At 50° FAHRENHEIT it becomes a soft and malleable solid, which has the lustre of polished silver; and at about the freezing point of water it becomes harder and brittle, and when broken in fragments, exhibits a crystallized texture, which in the microscope seems composed of beautiful facets of a perfect whiteness and high metallic splendour.

To be converted into vapour, it requires a temperature approaching that of the red heat; and when the experiment is conducted under proper circumstances, it is found unaltered after distillation.

It is a perfect conductor of electricity. When a spark from the VOLTAIC battery of 100 of 6 inches is taken upon a large globule in the atmosphere, the light is green, and combustion takes place at the point of contact only. When a small globule is used, it is completely dissipated with explosion accompanied by a most vivid flame, into alkaline fumes.

It is an excellent conductor of heat.

Resembling the metals in all these sensible properties, it is however remarkably different from any of them in specific gravity; I found that it rose to the surface of naphtha distilled from petroleum, and of which the specific gravity was .861 and it did not sink in double distilled naphtha, the specific gravity of which was about .770, that of water being considered as 1. The small quantities in which it is produced by the highest electrical powers, rendered it very difficult to determine this quality with minute precision. I endeavoured to gain approximations on the subject by comparing the weights of perfectly equal globules of the basis of potash and mercury. I used the very delicate balance of the Royal Institution, which when loaded with the quantities I employed, and of which the mercury never exceeded ten grains, is sensible at least to the $\frac{1}{2000}$ of a grain. Taking the mean of 4 experiments, conducted with great care, its specific gravity at 62° FAHRENHEIT, is to that of mercury as 10 to 223, which gives a proportion to that of water nearly as 6 to 10; so that it is the lightest fluid body

known. In its solid form it is a little heavier, but even in this state when cooled to 40° FAHRENHEIT, it swims in the double distilled naphtha.

The chemical relations of the basis of potash are still more extraordinary than its physical ones.

I have already mentioned its alkalization and combustion in oxygene gas.—It combines with oxygene slowly and without flame at all temperatures that I have tried below that of its vaporization.—But at this temperature combustion takes place, and the light is of a brilliant whiteness and the heat intense. When heated slowly in a quantity of oxygene gas not sufficient for its complete conversion into potash, and at a temperature inadequate to its inflammation, 400° FAHRENHEIT, for instance; its tint changes to that of a red brown, and when the heat is withdrawn, all the oxygene is found to be absorbed, and a solid is formed of a greyish colour, which partly consists of potash and partly of the basis of potash in a lower degree of oxygenation,—and which becomes potash by being exposed to water, or by being again heated in fresh quantities of air.

The substance consisting of the basis of potash combined with an under proportion of oxygene, may likewise be formed by fusing dry potash and its basis together under proper circumstances.—The basis rapidly loses its metallic splendour; the two substances unite into a compound, of a red brown colour when fluid, and of a dark grey hue when solid; and this compound soon absorbs its full proportion of oxygene when exposed to the air, and is wholly converted into potash.

And the same body is often formed in the analytical

experiments when the action of the electricity is intense, and the potash much heated.

The basis of potash when introduced into oxymuriatic acid gas burns spontaneously with a bright red light, and a white salt proving to be muriate of potash is formed.

When a globule is heated in hydrogene at a degree below its point of vaporization, it seems to dissolve in it, for the globule diminishes in volume, and the gas explodes with alkaline fumes and bright light, when suffered to pass into the air; but by cooling, this spontaneous detonating property is destroyed, and the basis is either wholly or principally deposited.

The action of the basis of potash on water exposed to the atmosphere is connected with some beautiful phenomena. When it is thrown upon water, or when it is brought into contact with a drop of water at common temperatures, it decomposes it with great violence, an instantaneous explosion is produced with brilliant flame, and a solution of pure potash is the result.

In experiments of this kind, an appearance often occurs similar to that produced by the combustion of phosphuretted hydrogene; a white ring of smoke, which gradually extends as it rises into the air.

When water is made to act upon the basis of potash out of the contact of air and preserved by means of a glass tube under naphtha, the decomposition is violent; and there is much heat and noise, but no luminous appearance, and the gas evolved when examined in the mercurial or water pneumatic apparatus is found to be pure hydrogene.

When a globule of the basis of potash is placed upon ice it

instantly burns with a bright flame, and a deep hole is made in the ice, which is found to contain a solution of potash.

The theory of the action of the basis of potash upon water exposed to the atmosphere, though complicated changes occur, is far from being obscure. The phenomena seem to depend on the strong attractions of the basis for oxygene and of the potash formed for water. The heat, which arises from two causes, decomposition and combination, is sufficiently intense to produce the inflammation. Water is a bad conductor of heat; the globule swims exposed to air; a part of it, there is the greatest reason to believe, is dissolved by the heated nascent hydrogene; and this substance being capable of spontaneous inflammation, explodes, and communicates the effect of combustion to any of the basis that may be yet uncombined.

When a globule confined out of the contact of air is acted upon by water, the theory of decomposition is very simple, the heat produced is rapidly carried off, so that there is no ignition; and a high temperature being requisite for the solution of the basis in hydrogene this combination probably does not take place, or at least it can have a momentary existence only.

The production of alkali in the decomposition of water by the basis of potash is demonstrated in a very simple and satisfactory manner by dropping a globule of it upon moistened paper tinged with turmeric. At the moment that the globule comes into contact with the water, it burns, and moves rapidly upon the paper, as if in search of moisture, leaving behind it a deep reddish brown trace, and acting upon the paper precisely as dry caustic potash.

So strong is the attraction of the basis of potash for oxygene,

and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are carefully purified.

In ether this decomposition is connected with an instructive result. Potash is insoluble in this fluid; and when the basis of potash is thrown into it, oxygene is furnished to it, and hydrogene gas disengaged, and the alkali as it forms renders the ether white and turbid.

In both these inflammable compounds the energy of its action is proportional to the quantity of water they contain, and hydrogene and potash are the constant result.

The basis of potash when thrown into solutions of the mineral acids, inflames and burns on the surface. When it is plunged by proper means beneath the surface enveloped in potash, surrounded by naphtha, it acts upon the oxygene with the greatest intensity, and all its effects are such as may be explained from its strong affinity for this substance. In sulphuric acid a white saline substance with a yellow coating, which is probably sulphate of potash surrounded by sulphur, and a gas which has the smell of sulphureous acid, and which probably is a mixture of that substance with hydrogene gas, are formed. In nitrous acid, nitrous gas is disengaged, and nitrate of potash formed.

The basis of potash readily combines with the simple inflammable solids, and with the metals; with phosphorus and sulphur, it forms compounds similar to the metallic phosphurets and sulphurets.

When it is brought in contact with a piece of phosphorus, and pressed upon, there is a considerable action: they become

fluid together, burn, and produce phosphate of potash. When the experiment is made under naphtha, their combination takes place without the liberation of any elastic matter, and they form a compound which has a considerably higher point of fusion than its two constituents, and which remains a soft solid in boiling naphtha. In its appearance it perfectly agrees with a metallic phosphuret, it is of the colour of lead, and when spread out, has a lustre similar to polished lead. When exposed to air at common temperatures, it slowly combines with oxygene, and becomes phosphate of potash. When heated upon a plate of platina, fumes exhale from it, and it does not burn till it attains the temperature of the rapid combustion of the basis of potash.

When the basis of potash is brought in contact with sulphur in fusion, in tubes filled with the vapour of naphtha, they combine rapidly with the evolution of heat and light, and a grey substance, in appearance like artificial sulphuret of iron, is formed, which if kept in fusion, rapidly dissolves the glass, and becomes bright brown. When this experiment is made in a glass tube hermetically sealed, no gas is liberated if the tube is opened under mercury; but when it is made in a tube connected with a mercurial apparatus, a small quantity of sulphuretted hydrogene is evolved, so that the phenomena are similar to those produced by the union of sulphur with the metals in which sulphuretted hydrogene is likewise disengaged, except that the ignition is stronger.* When the union is effected

* The existence of hydrogene in sulphur, is rendered very probable by the ingenious researches of M. Berthollet Jun. Annales de Chimie, Fevrier 1807 page 143. The fact is almost demonstrated by an experiment which I saw made by W. Clayfield, Esq. at Bristol, in 1799. Copper filings and powdered sulphur, in weight in the proportion of three

in the atmosphere, a great inflammation takes place, and sulphuret of potash is formed. The sulphuretted basis likewise gradually becomes oxygenated by exposure to the air, and is finally converted into sulphate.

The new substance produces some extraordinary and beautiful results with mercury. When one part of it is added to 8 or 10 parts of mercury in volume at 60° FAHRENHEIT, they instantly unite and form a substance exactly like mercury in colour, but which seems to have less coherence, for small portions of it appear as flattened spheres. When a globule is made to touch a globule of mercury about twice as large, they combine with considerable heat; the compound is fluid at the temperature of its formation; but when cool it appears as a solid metal, similar in colour to silver. If the quantity of the basis of potash is still farther increased, so as to be about $\frac{2}{100}$th the weight of the mercury, the amalgam increases in hardness, and becomes brittle. The solid amalgam, in which the basis is in the smallest proportion, seems to consist of about 1 part in weight of basis and 70 parts of mercury, and is very soft and malleable.

When these compounds are exposed to air, they rapidly absorb oxygene; potash which deliquesces is formed; and in a few minutes the mercury is found pure and unaltered.

When a globule of the amalgam is thrown into water, it

to one rendered very dry, were heated together in a retort, connected with a mercurial pneumatic apparatus. At the moment of combination a quantity of elastic fluid was liberated amounting to 9 or 10 times the volume of the materials employed, and which consisted of sulphuretted hydrogene mixed with sulphureous acid. The first mentioned product, there is every reason to believe, must be referred to the sulphur, the last probably to the copper, which it is easy to conceive may have become slightly and superficially oxidated during the processes of filing and drying by heat.

rapidly decomposes it with a hissing noise; potash is formed, pure hydrogene disengaged, and the mercury remains free.

The fluid amalgam of mercury and this substance dissolves all the metals I have exposed to it; and in this state of union, mercury acts on iron and platina.

When the basis of potash is heated with gold, or silver, or copper, in a close vessel of pure glass, it rapidly acts upon them; and when the compounds are thrown into water, this fluid is decomposed, potash formed, and the metals appear to be separated unaltered.

The basis of potash combines with fusible metal, and forms an alloy with it, which has a higher point of fusion than the fusible metal.

The action of the basis of potash upon the inflammable oily compound bodies, confirms the other facts of the strength of its attraction for oxygene.

On naphtha colourless and recently distilled, as I have already said, it has very little power of action; but in naphtha that has been exposed to the air it soon oxidates, and alkali is formed, which unites with the naphtha into a brown soap that collects round the globule.

On the concrete oils (tallow, spermaceti, wax, for instance), when heated, it acts slowly, coaly matter is deposited, a little gas* is evolved, and a soap is formed; but in these cases it is

* When a globule of the basis of potash is introduced into any of the fixed oils heated, the first product is pure hydrogene which arises from the decomposition of the water absorbed by the crust of potash during the exposure to the atmosphere. The gas evolved, when the globule is freed from this crust, I have found to be carbonated hydrogene requiring more than an equal bulk of oxygene gas for its complete saturation by explosion. I have made a great number of experiments, which it would be foreign to the object of this lecture to give in minute detail, on the agencies of the

necessary that a large quantity of the oil be employed. On the fluid fixed oils it produces the same effects, but more slowly.

By heat likewise it rapidly decomposes the volatile oils; alkali is formed, a small quantity of gas is evolved, and charcoal is deposited.

When the basis of potash is thrown into camphor in fusion, the camphor soon becomes blackened, no gas is liberated in the process of decomposition, and a saponaceous compound is formed; which seems to shew that camphor contains more oxygene than the volatile oils.

The basis of potash readily reduces metallic oxides when heated in contact with them. When a small quantity of the oxide of iron was heated with it, to a temperature approaching its point of distillation, there was a vivid action; alkali and grey metallic particles, which dissolved with effervescence in muriatic acid, appeared. The oxides of lead and the oxides of tin were revived still more rapidly; and when the basis of potash was in excess, an alloy was formed with the revived metal.

In consequence of this property, the basis of potash readily decomposes flint glass and green glass, by a gentle heat; alkali is immediately formed by oxygene from the oxides, which dissolves the glass, and a new surface is soon exposed to the agent.

basis of potash on the oils. Some anomalies occurred which led to the inquiry, and the result was perfectly conclusive. Olive oil, oil of turpentine, and naphtha when decomposed by heat, exhibited as products different proportions of charcoal, heavy inflammable gas, empyreumatic oily matter, and water, so that the existence of oxygene in them was fully proved; and accurate indications of the proportions of their elements might be gained by their decomposition by the basis of potash. Naphtha of all furnished least water and carbonic acid, and oil of turpentine the most.

At a red heat, even the purest glass is altered by the basis of potash: the oxygene in the alkali of the glass seems to be divided between the two bases, the basis of potash and the alkaline basis in the glass, and oxides, in the first degree of oxygenation, are the result. When the basis of potash is heated in tubes made of plate glass filled with the vapour of naphtha, it first acts upon the small quantity of the oxides of cobalt and manganese in the interior surface of the glass, and a portion of alkali is formed. As the heat approaches to redness, it begins to rise in vapour, and condenses in the colder parts of the tube; but at the point where the heat is strongest, a part of the vapour seems to penetrate the glass, rendering it of a deep red brown colour; and by repeatedly distilling and heating the substance in a close tube of this kind, it finally loses its metallic form, and a thick brown crust, which slowly decomposes water, and which combines with oxygene when exposed to air forming alkali, lines the interior of the tube, and in many parts is found penetrating through its substance.*

In my first experiments on the distillation of the basis of potash, I had great difficulty in accounting for these phenomena; but the knowledge of the substance it forms in its first degree of union with oxygene, afforded a satisfactory explanation.

* This is the obvious explanation in the present state of our knowledge; but it is more than probable that the silex of the glass likewise suffers some change, and probably decomposition. This subject I hope to be able to resume on another occasion.

5. *On the Properties and Nature of the Basis of Soda.*

The basis of soda, as I have already mentioned, is a solid at common temperatures. It is white, opaque, and when examined under a film of naphtha, has the lustre and general appearance of silver. It is exceedingly malleable, and is much softer than any of the common metallic substances. When pressed upon by a platina blade, with a small force, it spreads into thin leaves, and a globule of the $\frac{1}{10}$th or $\frac{1}{12}$th of an inch in diameter is easily spread over a surface of a quarter of an inch,* and this property does not diminish when it is cooled to 32° Fahrenheit.

It conducts electricity and heat in a similar manner to the basis of potash; and small globules of it inflame by the voltaic electrical spark, and burn with bright explosions.

Its specific gravity is less than that of water. It swims in oil of sassafras of 1.096, water being 1, and sinks in naphtha of specific gravity .861. This circumstance enabled me to ascertain the point with precision. I mixed together oil of sassafras and naphtha, which combine very perfectly, observing the proportions till I had composed a fluid, in which it remained at rest above or below; and this fluid consisted of nearly twelve parts naphtha, and five of oil of sassafras, which gives a specific gravity to that of water, nearly as nine to ten, or more accurately as .9348 to 1.

The basis of soda has a much higher point of fusion than the basis of potash; its parts begin to lose their cohesion at

* Globules may be easily made to adhere and form one mass by strong pressure: so that the property of welding, which belongs to iron and platina at a white heat only, is possessed by this substance at common temperatures.

about 120° FAHRENHEIT, and it is a perfect fluid at about 180°, so that it readily fuses under boiling naphtha.

I have not yet been able to ascertain at what degree of heat it is volatile; but it remains fixed in a state of ignition at the point of fusion of plate glass.

The chemical phenomena produced by the basis of soda, are analogous to those produced by the basis of potash; but with such characteristic differences as might be well expected.

When the basis of soda is exposed to the atmosphere, it immediately tarnishes, and by degrees becomes covered with a white crust, which deliquesces much more slowly than the substance which forms on the basis of potash. It proves, on minute examination, to be pure soda.

The basis of soda combines with oxygene slowly, and without luminous appearance at all common temperatures; and when heated, this combination becomes more rapid; but no light is emitted till it has acquired a temperature nearly that of ignition.

The flame that it produces in oxygene gas is white, and it sends forth bright sparks, occasioning a very beautiful effect; in common air, it burns with light of the colour of that produced during the combustion of charcoal, but much brighter.

The basis of soda when heated in hydrogene, seemed to have no action upon it. When introduced into oxymuriatic acid gas, it burnt vividly with numerous scintillations of a bright red colour. Saline matter was formed in this combustion, which, as might have been expected, proved to be muriate of soda.

Its operation upon water offers most satisfactory evidence of its nature. When thrown upon this fluid, it produces a

violent effervescence, with a loud hissing noise; it combines with the oxygene of the water to form soda, which is dissolved, and its hydrogene is disengaged. In this operation there is no luminous appearance; and it seems probable that even in the nascent state hydrogene is incapable of combining with it.*

When the basis of soda is thrown into hot water, the decomposition is more violent, and in this case a few scintillations are generally observed at the surface of the fluid; but this is owing to small particles of the basis, which are thrown out of the water sufficiently heated, to burn in passing through the atmosphere. When, however, a globule is brought in contact with a small particle of water, or with moistened paper, the heat produced (there being no medium to carry it off rapidly) is usually sufficient for the accension of the basis.

The basis of soda acts upon alcohol and ether precisely in a similar manner with the basis of potash. The water that they contain is decomposed; soda is rapidly formed, and hydrogene disengaged.

The basis of soda, when thrown upon the strong acids, acts upon them with great energy. When nitrous acid is employed, a vivid inflammation is produced; with muriatic and sulphuric acid, there is much heat generated, but no light.

When plunged, by proper means, beneath the surface of the acids, it is rapidly oxygenated; soda is produced, and the other educts are similar to those generated by the action of the basis of potash.

* The more volatile metals only seem capable of uniting with hydrogene; a circumstance presenting an analogy.

With respect to the fixed and volatile oils and naphtha in their different states, there is a perfect coincidence between the effects of the two new substances, except in the difference of the appearances of the saponaceous compounds formed: those produced by the oxydation and combination of the basis of soda being of a darker colour, and apparently less soluble.

The basis of soda, in its degrees of oxydation, has precisely similar habits with the basis of potash.

When it is fused with dry soda, in certain quantities, there is a division of oxygene between the alkali and the base; and a deep brown fluid is produced, which becomes a dark grey solid on cooling, and which attracts oxygene from the air, or which decomposes water, and becomes soda.

The same body is often formed in the analytical processes of decomposition, and it is generated when the basis of soda is fused in tubes of the purest plate glass.

There is scarcely any difference in the visible phenomena of the agencies of the basis of soda, and that of potash on sulphur, phosphorus, and the metals.

It combines with sulphur in close vessels filled with the vapour of naphtha with great vividness, with light, heat, and often with explosion from the vaporization of a portion of sulphur, and the disengagement of sulphuretted hydrogene gas. The sulphuretted basis of soda is of a deep grey colour.

The phosphuret has the appearance of lead, and forms phosphate of soda by exposure to air, or by combustion.

The basis of soda in the quantity of $\frac{1}{10}$, renders mercury a fixed solid of the colour of silver, and the combination is attended with a considerable degree of heat.

It makes an alloy with tin, without changing its colour, and

it acts upon lead and gold when heated. I have not examined its habitudes with any other metals, but in its state of alloy, it is soon converted into soda by exposure to air, or by the action of water, which it decomposes with the evolution of hydrogene.

The amalgam of mercury and the basis of soda, seems to form triple compounds with other metals. I have tried iron and platina, which I am inclined to believe remain in combination with the mercury, when it is deprived of the new substance by exposure to air.

The amalgam of the basis of soda and mercury likewise combines with sulphur and forms a triple compound of a dark grey colour.

VI. *On the Proportions of the peculiar Bases and Oxygene in Potash and Soda.*

The facility of combustion of the bases of the alkalies, and the readiness with which they decomposed water, offered means fully adequate for determining the proportions of their ponderable constituent parts.

I shall mention the general methods of the experiments, and the results obtained by the different series, which approach as near to each other as can be expected in operations performed on such small quantities of materials.

For the process in oxygene gas, I employed glass tubes containing small trays made of thin leaves of silver or other noble metals, on which the substance to be burnt, after being accurately weighed or compared with a globule of mercury,

equal in size* was placed: the tube was small at one end, curved, and brought to a fine point, but suffered to remain open; and the other end was fitted to a tube communicating with a gazometer, from which the oxygene gas was introduced, for neither water nor mercury could be used for filling the apparatus. The oxygene gas was carried through the tube till it was found that the whole of the common air was expelled. The degree of its purity was ascertained by suffering a small quantity to pass into the mercurial apparatus. The lower orifice was then hermetically sealed by a spirit lamp, and the upper part drawn out and finally closed, when the aperture was so small, as to render the temperature employed incapable of materially influencing the volume of the gas; and when the whole arrangement was made, the combination was effected by applying heat to the glass in contact with the metallic tray.

In performing these experiments many difficulties occurred. When the flame of the lamp was immediately brought to play upon the glass, the combustion was very vivid, so as sometimes to break the tube; and the alkali generated partly rose in white fumes, which were deposited upon the glass.

When the temperature was slowly raised, the bases of the alkalies acted upon the metallic tray and formed alloys, and in this state it was very difficult to combine them with their full proportion of oxygene; and glass alone could not be employed on account of its decomposition by the alkaline bases; and porcelain is so bad a conductor of heat, that it was

* When the globules were very small, the comparison with mercury, which may be quickly made by means of a micrometer, was generally employed as the means of ascertaining the weight: for in this case the globule could be immediately introduced into the tube, and the weight of mercury ascertained at leisure.

not possible to raise it to the point required for the process, without softening the glass.

In all cases the globules of the alkaline bases were carefully freed from naphtha before they were introduced; of course a slight crust of alkali was formed before the combustion, but this could not materially affect the result; and when such a precaution was not used, an explosion generally took place from the vaporization and decomposition of the film of naphtha surrounding the globule.

After the combustion, the absorption of gas was ascertained, by opening the lower point of the tube under water or mercury. In some cases the purity of the residual air was ascertained, in others the alkali formed in the tray was weighed.

From several experiments on the synthesis of potash by combustion, I shall select two, which were made with every possible attention to accuracy, and under favourable circumstances, for a mean result.

In the first experiment 0.12 grains of the basis were employed. The combustion was made upon platina, and was rapid and complete; and the basis appeared to be perfectly saturated, as no disengagement of hydrogene took place when the platina tray was thrown into water. The oxygene gas absorbed equalled in volume 190 grain measures of quicksilver; barometer being at 29.6 inches, thermometer 62° FAHRENHEIT; and this reduced to a temperature of 60° FAHRENHEIT, and under a pressure equal to that indicated by 30 inches,* would become 186.67 measures, the weight of which

* In the correction for temperature, the estimations of DALTON and GAY LUSSAC are taken, which make gasses expand about $\frac{1}{210}$ of the primitive volume for every degree of FAHRENHEIT.

would be about .0184 grains troy;* but .0184 : .1384 :: 13.29 : 100; and according to this estimation 100 parts of potash will consist of 86.7 basis, and 13.3 oxygene nearly.

In the second experiment .07 grains of the basis absorbed at temperature 63° of FAHRENHEIT, and under pressure equal to 30.1 barometer inches, a quantity of oxygene equal in volume to 121 grain measures of mercury, and the proper corrections being made as in the former case, this gas would weigh .01189 grains.

But as .07 + .01189 = .08189 : .07 : : 100 : 85.48 nearly, and 100 parts of potash will consist of 85.5 of basis and 14.5 of oxygene nearly. And the mean of the two experiments will be 86.1 of basis to 13.9 of oxygene for 100 parts.

In the most accurate experiment that I made on the combustion of the basis of soda .08 parts of the basis absorbed a quantity of oxygene equal to 206 grain measures of mercury; the thermometer being at 56° FAHRENHEIT; and the barometer at 29.4; and this quantity, the corrections being made as before for the mean temperature and pressure, equals about .02 grains of oxygene.

And as .08 + .02 = .10 : .08, : : 100 : 80, and 100 parts of soda according to this estimation will consist of 80 basis to 20 of oxygene.

In all cases of slow combustion, in which the alkalies were not carried out of the tray, I found a considerable increase of weight, but as it was impossible to weigh them except in the

* From experiments that I made in 1799, on the specific gravity of oxygene gas, it would appear that its weight is to that of water as 1 to 743, and to that of quicksilver as 1 to 10142. *Researches Chem.* and *Phil.* p. 93; and with this estimation, that deducible from the late accurate researches of Messrs. ALLEN and PEPYS on the Combustion of the Diamond almost precisely agrees. Phil. Trans. 1807, page 273.

atmosphere, the moisture attracted rendered the results doubtful; and the proportions from the weight of the oxygene absorbed are more to be depended on. In the experiments in which the processes of weighing were most speedily performed, and in which no alkali adhered to the tube, the basis of potash gained nearly 2 parts for 10, and that of soda between 3 and 4 parts.

The results of the decomposition of water by the bases of the alkalies were much more readily and perfectly obtained than those of their combustion.

To check the rapidity of the process, and, in the case of potash, to prevent any of the basis from being dissolved, I employed the amalgams with mercury. I used a known weight of the bases, and made the amalgams under naphtha, using about two parts of mercury in volume to one of basis.

In the first instances I placed the amalgams under tubes filled with naphtha, and inverted in glasses of naphtha, and slowly admitted water to the amalgam at the bottom of the glass; but this precaution I soon found unnecessary, for the action of the water was not so intense but that the hydrogene gas could be wholly collected.

I shall give an account of the most accurate experiments made on the decomposition of water by the bases of potash and soda.

In an experiment on the basis of potash conducted with every attention that I could pay to the minutiæ of the operations, hydrogene gas, equal in volume to 298 grains of mercury, were disengaged by the action of .08 grains of the basis of potash which had been amalgamated with about 8 grains of mercury. The thermometer at the end of the process

indicated a temperature of 56° FAHRENHEIT, and the barometer an atmospheric pressure equal to 29.6 inches.

Now this quantity of hydrogene* would require for its combustion a volume of oxygene gas about equal to that occupied by 154.9 grains of mercury, which gives the weight of oxygene required to saturate the .08 grains of the basis of potash at the mean temperature and pressure nearly .0131 grains. And .08 + .0151 = .0951 : .08 :: 100 : 84.1 nearly.

And according to these indications 100 parts of potash consist of about 84 basis and 16 oxygene.

In an experiment on the decomposition of water by the basis of soda, the mercury in the barometer standing at 30.4 inches, and in the thermometer at 52° FAHRENHEIT, the volume of hydrogene gas evolved by the action of .054 grains of basis equalled that of 326 grains of quicksilver. Now this at the mean temperature and pressure would require for its conversion into water, .0172 of oxygene, and .054 + .0172 = .0712 : .054 : : 100 : 76 nearly; and according to these indications, 100 parts of soda consist of nearly 76 basis, and 24 oxygene.

In another experiment made with very great care, .052 of the basis of soda were used; the mercury in the barometer was at 29.9 inches, and that in the thermometer at 58° FAHRENHEIT. The volume of hydrogene evolved was equal to that of 302 grains of mercury; which would demand for its saturation by combustion, at the mean temperature and pressure .01549 grains of oxygene; and 100 parts of soda, according to this proportion, would consist nearly of 77 basis, and 23 oxygene.

* *Researches Chem. and Phil.* page 287.

The experiments which have been just detailed, are those in which the largest quantities of materials were employed; I have compared their results, however, with the results of several others, in which the decomposition of water was performed with great care, but in which the proportion of the bases was still more minute: the largest quantity of oxygene indicated by these experiments was, for potash 17, and for soda 26 parts in 100, and the smallest 13, and 19; and comparing all the estimations, it will probably be a good approximation to the truth, to consider potash as composed of about 6 parts basis and 1 of oxygene; and soda, as consisting of 7 basis and 2 oxygene.

VII. *Some general Observations on the Relations of the Bases of Potash and Soda to other Bodies.*

Should the bases of potash and soda be called metals? The greater number of philosophical persons to whom this question has been put, have answered in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination.

Their low specific gravity does not appear a sufficient reason for making them a new class; for amongst the metals themselves there are remarkable differences in this respect, platina being nearly four times as heavy as tellurium;* and in

* Tellurium is not much more than six times as heavy as the basis of soda. There is great reason to believe that bodies of a similar chemical nature to the bases of potash and soda will be found of intermediate specific gravities between them and the lightest of the common metals. Of this subject, I shall treat again in the text in some of the following pages.

the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.

On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination which, by common consent, has been applied to other newly discovered metals, and which, though originally Latin, is now naturalized in our language.

Potasium and Sodium are the names by which I have ventured to call the two new substances: and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an error; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country, upon the methods of derivation, and the one I have adopted has been the one most generally approved. It is perhaps more significant than elegant. But it was not possible to found names upon specific properties not common to both; and though a name for the basis of soda might have been borrowed from the Greek, yet an analogous one could not have been applied to that of potash, for the ancients do not seem to have distinguished between the two alkalies.

The more caution is necessary in avoiding any theoretical expression in the terms, because the new electro-chemical phenomena that are daily becoming disclosed, seem distinctly to shew that the mature time for a complete generalization of chemical facts is yet far distant; and though, in the explanations of the various results of experiments that have been detailed, the antiphlogistic solution of the phenomena has

been uniformly adopted, yet the motive for employing it has been rather a sense of its beauty and precision, than a conviction of its permanency and truth.

The discovery of the agencies of the gasses destroyed the hypothesis of STAHL. The knowledge of the powers and effects of the etherial substances may at a future time possibly act a similar part with regard to the more refined and ingenious hypothesis of LAVOISIER; but in the present state of our knowledge, it appears the best approximation that has been made to a perfect logic of chemistry.

Whatever future changes may take place in theory, there seems however every reason to believe that the metallic bases of the alkalies, and the common metals, will stand in the same arrangement of substances; and as yet we have no good reasons for assuming the compound nature of this class of bodies.*

The experiments in which it is said that alkalies, metallic oxides, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner;† for distilled water, as I have endeavoured

* A phlogistic chemical theory might certainly by defended, on the idea that the metals are compounds of certain unknown bases with the same matter as that existing in hydrogene; and the metallic oxides, alkalies and acids compounds of the same bases with water;—but in this theory more unknown principles would be assumed than in the generally received theory. It would be less elegant and less distinct. In my first experiments on the distillation of the basis of potash finding hydrogene generally produced, I was led to compare the phlogistic hypothesis with the new facts, and I found it fully adequate to the explanation. More delicate researches however afterwards proved that in the cases when inflammable gasses appeared, water, or some body in which hydrogene is admitted to exist, was present.

† The explanation of VAN HELMONT of his fact of the production of earth in the growth of the willow, was completely overturned by the researches of WOODWARD. Phil. Trans. Vol. XXI. page. 193.

to show,* may contain both saline and metallic impregnations; and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds.

In the common processes of nature, all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of the alkalies, and earths, with mineral acids, generally abound in soils. From the decomposition of basaltic, porphyritic,† and granitic rocks, there is a constant supply of earthy alkaline and ferruginous materials to the surface of the earth. In the sap of all plants that have been examined, certain neutrosaline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them into simple elements.

The conclusions which M. BRACONNOT has very lately drawn from his ingenious experiments, Annales de Chemie, Fevrier 1807, page 187, are rendered of little avail in consequence of the circumstances stated in the text. In the only case of vegetation in which the free atmosphere was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances which might afford carbon, or various inflammable matters. Carbonaceous matter exists in several stones which afford a whitish or greyish powder; and when in a stone, the quantity of carbonate of lime is very small in proportion to the other earthy ingredients, it is scarcely acted on by acids.

* Bakerian Lecture, 1806, page 8.

† In the year 1804, for a particular purpose of geological enquiry, I made an analysis of the porcelain clay of St. Stevens, in Cornwall, which results from the decomposition of the feldspar of fine-grained granite. I could not detect in it the smallest quantity of alkali. In making some experiments on specimens of the undecompounded rock taken from beneath the surface, there were evident indications of the presence of a fixed alkali, which seemed to be potash. So that it is very probable that the decomposition depends on the operation of water and the carbonic acid of the atmosphere on the alkali forming a constituent part of the chrystalline matter of the feldspar, which may disintegrate from being deprived of it.

VIII. *On the Nature of Ammonia and alkaline Bodies in general; with Observations on some prospects of Discovery offered by the preceding Facts.*

Ammonia is a substance, the chemical composition of which has always been considered of late years as most perfectly ascertained, and the apparent conversion of it into hydrogene and nitrogene, in the experiments of SCHEELE, PRIESTLEY, and the more refined and accurate experiments of BERTHOLLET, had left no doubt of its nature in the minds of the most enlightened chemists.

All new facts must be accompanied however by a train of analogies, and often by suspicions with regard to the accuracy of former conclusions. As the two fixed alkalies contain a small quantity of oxygene united to peculiar bases, may not the volatile alkali likewise contain it? was a query which soon occurred to me in the course of enquiry; and in perusing the accounts of the various experiments made on the subject, some of which I had carefully repeated, I saw no reason to consider the circumstance as impossible. For supposing hydrogene and nitrogene to exist in combination with oxygene in low proportion, this last principle might easily disappear in the analytical experiments of decomposition by heat and electricity, in water deposited upon the vessels employed or dissolved in the gasses produced.

Of the existence of oxygene in volatile alkali I soon satisfied myself. When charcoal carefully burnt and freed from moisture was ignited by the VOLTAIC battery of the power of 250 of 6 and 4 inches square, in a small quantity of very pure

ammoniacal gas:* a great expansion of the æriform matter took place, and a white substance formed, which collected on the sides of the glass tube employed in the process; and this matter, exposed to the action of diluted muriatic acid, effervesced, so that it was probably carbonate of ammonia.

A process of another kind offered still more decisive results. In this the two mercurial gazometers of the invention of Mr. PEPYS, described in No XIV. of the Phil. Trans. for 1807, were used with the same apparatus, as that employed by Messrs. ALLEN and PEPYS for the combustion of the diamond, and these gentlemen kindly assisted in the experiment.

Very pure ammoniacal gas was passed over iron wire ignited in a platina tube, and two curved glass tubes were so arranged as to be inserted into a freezing mixture; and through one of these tubes the gas entered into the platina tube, and through the other, it passed from the platina tube into the airholder arranged for its reception.

The temperature of the atmosphere was 55°; but it was observed that no sensible quantity of water was deposited in the cooled glass tube transmitting the unaltered ammonia, but in that receiving it after its exposure to heat, moisture was very distinct, and the gas appeared in the airholder densely clouded.

* The apparatus in which this experiment was made is described in page 214 Journal of the Royal Institution. The gas was confined by mercury which had been previously boiled to expel any moisture that might adhere to it. The ammonia had been exposed to the action of dry pure potash, and a portion of it equal in volume to 10980 grains of mercury, when acted on by distilled water, left a residuum equal to 9 grains of mercury only. So that the gas, there is every reason to believe, contained no foreign æriform matter; for even the minute residuum may be accounted for by supposing it derived from air dissolved in the water.

This circumstance seems distinctly to prove the formation of water in this operation for the decomposition of ammonia; unless indeed it be asserted that the hydrogene and nitrogene gasses evolved hold less water in solution or suspension than the ammonia decomposed, an idea strongly opposed by the conclusions of Mr. DALTON* and the experiments of Messrs. DESORMES and CLEMENT.†

After the gas had been passed several times through the ignited tube from one gazometer to the other, the results were examined. The iron wire became converted superficially into oxide, and had gained in weight $\frac{4.4}{100}$ parts of a grain, about $\frac{4}{10}$ of a grain of water were collected from the cooled glass tubes by means of filtrating paper, and 33.8 cubic inches of gas were expanded into 55.3 cubic inches, and by detonation with oxygene it was found that the hydrogene gas in these was to the nitrogene as 3.2. to 1 in volume.

It will be useless to enter into the more minute details of this experiment, as no perfectly accurate data for proportions can be gained from them; for the whole of the ammonia was not decomposed, and as the gas had been prepared by being sent from a heated mixture of sal ammoniac and quicklime, into the airholder, it was possible that some solution of ammonia might have been deposited, which, by giving out new gas during the operation, would increase the absolute quantity of the material acted upon.

In examining the results of M. BERTHOLLET's‡ elaborate experiments on the decomposition of ammonia by electricity, I

* Manchester Memoirs, Vol. V. Part II. page. 535, 1785.

† Annales de Chemie, Vol. XLII. p. 125.

‡ *Mémoires de l'Academie*, 1785, page 324.

was surprised to find that the weight of the hydrogene and nitrogene produced, rather exceeded than fell short of that of the ammonia considered as decomposed, which was evidently contradictory to the idea of its containing oxygene. This circumstance, as well as the want of coincidence between the results and those of PRIESTLEY and VAN MARUM on the same subject, induced me to repeat the process of the electrization of ammonia, and I soon found that the quantities of the products in their relations to the apparent quantity of gas destroyed were influenced by many different causes.

Ammonia procured over dry mercury from a mixture of dry lime and muriate of ammonia, I found deposited moisture upon the sides of the vessel in which it was collected, and in passing the gas into the tube for electrization, it was not easy to avoid introducing some of this moisture, which must have been a saturated solution of ammonia, at the same time.

In my first trials made upon gas, passed immediately from the vessel in which it had been collected into the apparatus, I found the expansion of 1 of ammonia vary in different instances from 2.8 to 2.2 measures, but the proportions of the nitrogene and hydrogene appeared uniform, as determined by detonation of the mixed gas with oxygene, and nearly as 1 to 3 in volume.

To exclude free moisture entirely, I carefully prepared ammonia in a mercurial airholder, and after it had been some hours at rest, passed a quantity of it into the tube for decomposition, which had been filled with dry mercury. In this case 50 parts became 103 parts by electrization, and there was still reason to suspect sources of error.

I had used iron wires not perfectly free from rust, for

taking the spark, and a black film from the mercury appeared on the sides of the tube. It was probable that some ammonia had been absorbed by the metallic oxides both upon the iron and the mercury, which might again have been given out in the progress of the operation.

I now used recently distilled mercury, which did not leave the slightest film on the glass tube, and wires of platina. The ammonia had been exposed to dry caustic potash, and proved to be equally pure with that mentioned in page 36. 60 measures of it, each equal to a grain of water, were electrized till no farther expansion could be produced, the gas filled a space equal to that occupied by 108 grains of water. The thermometer in this experiment was at 56°, and the barometer at 30.1 inches. The wire of platina transmitting the spark was slightly tarnished.* The 108 measures of gas carefully analyzed, were found to consist of 80 measures in volume of hydrogene, and 28 measures of nitrogene.

The results of an experiment that I made in 1799,† give the weight of 100 cubic inches of ammonia, as 18.18 grains at the mean temperature and pressure. I had reasons however for suspecting that this estimation might be somewhat too low, and on mentioning the circumstance to Messrs. ALLEN and PEPYS, they kindly undertook the examination of the subject, and Mr. ALLEN soon furnished me with the following data. "In the first experiment 21 cubic inches of ammonia weighed 4.05 grains; in a second experiment the same quantity weighed 4.06 grains, barometer 30.65. thermometer 54° FAHRENHEIT."

* This most probably was owing to oxydation. When platina is made positive in the VOLTAIC circuit in contact with solution of ammonia, it is rapidly corroded. This is an analogous instance.

† Researches Chem. and Phil. p. 62.

Now if the corrections for temperature and pressure be made for these estimations, and a mean taken, 100 cubic inches of ammonia will weigh 18.67 grains, barometer being at 30, and thermometer at 60° FAHRENHEIT; and if the quantity used in the experiment of decomposition be calculated upon 25 cubic inches, 60 will weigh 11.2 grains. But the hydrogene gas evolved equal to 80 will weigh 1.93* grains, and the nitrogene equal to 28,† 8.9. And 11.2, grains — 1.9 + 8.9 = 10.2. and 11.2 — 10.2. = 1, all the estimations being made according to the standard temperature and pressure.

So that in this experiment on the decomposition of ammonia, the weight of the gasses evolved is less by nearly $\frac{1}{11}$ than that of the ammonia employed; and this loss can only be ascribed to the existence of oxygene in the alkali; part of which probably combined with the platina wires employed for electrization, and part with hydrogene.

After these ideas the oxygene in ammonia cannot well be estimated at less than 7 or 8 parts in the hundred; and it possibly exists in a larger proportion as the gasses evolved may contain more water than the gas decomposed, which of course would increase their volume and their absolute weight.‡

In supposing ammonia a triple compound of nitrogene,

* LAVOISIER's Elements, p. 569. A cubical inch of hydrogene is considered as weighing .0239.

† Researches Chem. and Phil. page 9. From my experiments 100 cubical inches of nitrogene weigh at the standard temperature and pressure, 29.6 grains.

‡ In the present state of our knowledge, perfectly correct data for proportions cannot probably be gained in any experiments on the decomposition of ammonia, as it seems impossible to ascertain the absolute quantity of water in this gas, for electrization, according to Dr HENRY's ingenious researches, offers the only means known of ascertaining the quantity of water in gasses.

hydrogene, and oxygene, it is no less easy to give a rational account of the phænomena of its production and decomposition, than in adopting the generally received hypothesis of its composition.

Oxygene, hydrogene, and nitrogene are always present in cases in which volatile alkali is formed; and it usually appears during the decomposition of bodies in which oxygene is loosely attached, as in that of the compounds of oxygene and nitrogene dissolved in water.

At common temperatures under favourable circumstances, the three elements may be conceived capable of combining and of remaining in union: but at the heat of ignition the affinity of hydrogene for oxygene prevails over the complex attraction, water is formed, and hydrogene and nitrogene are evolved; and according to these conclusions, ammonia will bear the same relations to the fixed alkalies, as the vegetable acids with compound bases do to the mineral ones with simple bases.

Oxygene then may be considered as existing in, and as forming, an element in all the true alkalies; and the principle of acidity of the French nomenclature, might now likewise be called the principle of alkalescence.

From analogy alone it is reasonable to expect that the alkaline earths are compounds of a similar nature to the fixed alkalies, peculiar highly combustible metallic bases united to oxygene. I have tried some experiments upon barytes and strontites; and they go far towards proving that this must be the case. When barytes and strontites, moistened with water, were acted upon by the power of the battery of 250 of 4 and 6, there was a vivid action and a brilliant light at both points of communication, and an inflammation at the negative point.

In these cases the water might possibly have interfered. Other experiments gave however more distinct results.

Barytes and strontites, even when heated to intense whiteness, in the electrical circuit by a flame supported by oxygene gas, are non-conductors; but by means of combination with a very small quantity of boracic acid, they become conductors; and in this case inflammable matter, which burns with a deep red light in each instance, is produced from them at the negative surface. The high temperature has prevented the success of attempts to collect this substance; but there is much reason to believe that it is the basis of the alkaline earth employed.

Barytes and strontites have the strongest relations to the fixed alkalies of any of the earthy bodies;* but there is a chain of resemblances, through lime, magnesia, glucina, alumina, and silex. And by the agencies of batteries sufficiently strong, and by the application of proper circumstances, there is no small reason to hope, that even these refractory bodies will yield their elements to the methods of analysis by electrical attraction and repulsion.

In the electrical circuit we have a regular series of powers, of decomposition from an intensity of action, so feeble as scarcely to destroy the weakest affinity existing between the parts of a saline neutral compound, to one sufficiently

* The similarity between the properties of earths and metallic oxides, was noticed in the early periods of chemistry. The poisonous nature of barytes, and the great specific gravity of this substance as well as of strontites, led LAVOISIER to the conjecture that they were of a metallic nature. That metals existed in the fixed alkalies seems however never to have been suspected. From their analogy to ammonia, nitrogene and hydrogene have been supposed to be amongst their elements. It is singular, with regard to this class of bodies, that those most unlike metallic oxides are the first which have been demonstrated to be such.

energetic to separate elements in the strongest degree of union, in bodies undecomposable under other circumstances.

When the powers are feeble, acids and alkalies, and acids and metallic oxides, merely separate from each other; when they are increased to a certain degree, the common metallic oxides and the compound acids are decomposed; and by means still more exalted, the alkalies yield their elements. And as far as our knowledge of the composition of bodies extends, all substances attracted by positive electricity, are oxygene, or such as contain oxygene in excess; and all that are attracted by negative electricity, are pure combustibles, or such as consist chiefly of combustible matter.

The idea of muriatic acid, fluoric acid, and boracic acid containing oxygene, is highly strengthened by these facts. And the general principle confirms the conjecture just stated concerning the nature of the earths.

In the electrization of boracic acid moistened with water, I find that a dark coloured combustible matter is evolved at the negative surface; but the researches upon the alkalies have prevented me from pursuing this fact, which seems however to indicate a decomposition.

Muriatic acid and fluoric acid in their gaseous states are non-conductors: and as there is every reason to believe that their bases have a stronger attraction for oxygene than water, there can be little hope of decomposing them in their aqueous solutions, even by the highest powers. In the electrization of some of their combinations there is however a probability of success.

An immense variety of objects of research is presented in the powers and affinities of the new metals produced from the alkalies.

In themselves they will undoubtedly prove powerful agents for analysis; and having an affinity for oxygene stronger than any other known substances, they may possibly supersede the application of electricity to some of the undecompounded bodies.

The basis of potash I find oxidates in carbonic acid and decomposes it, and produces charcoal when heated in contact with carbonate of lime. It likewise oxidates in muriatic acid; but I have had no opportunity of making the experiment with sufficient precision to ascertain the results.

In sciences kindred to chemistry, the knowledge of the nature of the alkalies, and the analogies arising in consequence, will open many new views; they may lead to the solution of many problems in geology, and shew that agents may have operated in the formation of rocks and earths which have not hitherto been suspected to exist.

It would be easy to pursue the speculative part of this enquiry to a great extent, but I shall refrain from so occupying the time of the Society, as the tenour of my object in this lecture has not been to state hypotheses, but to bring forward a new series of facts.

II. *On the Structure and Uses of the Spleen.* By Everard Home, Esq. F. R. S.

Read November 26, 1807.

In bringing forward a fact of so much importance, as a communication between the cardiac portion of the stomach and the circulation of the blood, through the medium of the spleen, I shall not take up the time of the Society by offering any preliminary observations, but state the circumstances which led to the discovery, and the experiments by which the different facts have been ascertained.

During the investigation of the functions of the stomach, (in which I have been lately engaged,) it was found that while digestion is going on, there is a separation between the cardiac and pyloric portions, either by means of a permanent or muscular contraction. This fact placed the process of digestion in a new light, and led me to consider in what way the quantities of different liquors, which are so often taken into the stomach, can be prevented from being mixed with the half digested food, and interfering with the formation of chyle.

Pursuing this enquiry, I found that the fluids are principally contained in the cardiac portion, and the food that has reached the pyloric portion is usually of one uniform consistence, so that the fluids beyond what are necessary for digestion would appear to be carried out of the stomach, without ever reaching

so far as the pylorus. To ascertain the truth of this opinion is the object of the present Paper.

The lymphatic vessels of the stomach are numerous, but they are equally or more so in the other viscera. Many circumstances appeared to render it probable that the spleen is the route by which liquids are conveyed. The more I considered the subject, new reasons in favour of this opinion crowded on my mind, so as almost to enforce conviction, and made me set about devising various methods, by which its truth or falsehood might be established.

The first point to be decided was, whether the liquids received into the stomach do escape in any considerable quantity, when prevented from passing out at the pylorus.

This was ascertained by the following experiment, made October 31, 1807, with the assistance of Mr. Brodie, Mr. W. Brande, and Mr. Clift.

The pylorus of a small dog was secured by a ligature, and a few minutes afterwards five ounces by measure, of an infusion of indigo in water, of the temperature of the atmosphere, were injected by the mouth into the stomach. At the end of half an hour the dog became sick, and brought up by vomiting 2 ounces of a nearly colourless fluid. The dog was immediately killed, and the different parts were examined. The pylorus was found completely secured by the ligature, so that nothing could pass in that direction. The pyloric portion of the stomach was found empty and contracted; the cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no colour, the indigo being completely separated from it, and spread over the surface of the internal mem-

brane. Of the five ounces of water thrown into the stomach, two were brought up by vomiting, and one only remained; two ounces had therefore escaped in the course of half an hour. As the stomach contained two ounces of solid food at the time the experiment was made, it is reasonable to suppose that there was also some liquid in it, and in this case the whole quantity that escaped must have exceeded two ounces. On examining the external covering of the stomach, and along the course of the vasa brevia, where the absorbents usually pass, none were discovered, so that these vessels were not at that time carrying any liquid.

The spleen was turgid, unusually large, and its external surface very irregular; when cut into, small cells were every where met with containing a watery fluid, and occupying a considerable portion of its substance. This appearance, which I had never seen before, made me enquire, if it had been taken notice of by others, and endeavour to ascertain the circumstances under which it is produced. The following statement contains the information which I have received on this subject.

Malpighi appears to be the first anatomist, who had any particular knowledge of the structure of the spleen. He describes its capsule, and a network which pervades every part of the substance. He mentions a number of small glands, which are hollow, and surrounded by arterial zones, but he had never been able to trace any venal branches into them. He believed that there was a cellular structure in the spleen containing red blood, interposed between the arteries and veins; this led him to adopt a theory that the network was muscular, and by

its action propelled the blood, so that there was a systole and diastole in the spleen, as in the heart.

Stukely, in his Gulstonian lecture, has very closely copied Malpighi, without giving any additional information.

Cuvier, the latest writer on this subject, in his *Leçons d'Anatomie comparée*, corrects the error of Malpighi respecting the nature of the network, which he states to be composed of elastic ligament, and says that there are small corpuscles, whose use is unknown, and which disappear when the blood vessels are minutely injected.

In the course of the present investigation, I have examined the spleen after death, under the ordinary circumstances, and have found the appearances described by Cuvier. I have also examined it frequently immediately after the stomach had received unusual quantities of liquids, and in that state have found invariably, that the corpuscles of Cuvier, which were the glands of Malpighi, are distinct cells, containing a fluid, which escapes when the cells are punctured, and renders their membranous coat visible, so that it would appear that the distension of these cells is connected with the state of the stomach, and therefore only takes place occasionally; and that the elastic capsule by which the spleen is surrounded adapts the organ to these changes in its volume.

On examining further into the structure of the spleen, in which I have been materially assisted by Mr. Brodie, the following facts have been ascertained.

In the spleen of the bullock, horse, and hog, the cells, when the arteries and veins are injected with coloured size, are seen to have numerous arterial branches ramifying in their coats,

but no venal ones, which confirms the statement of MALPIGHI; and when the cells are empty and contracted, and the blood-vessels filled to a great degree of minuteness, the appearance of cells is entirely lost, as stated by CUVIER.

When the cells were in a distended state, their cavities in a great many instances were very distinct, having been laid open in making a section of the spleen. The intermediate parts of the spleen are but sparingly supplied with arterial branches, and the smaller ones do not appear to have any particular distribution.

When the veins only are injected their branches appear more numerous, and larger than those of the arteries, making the whole substance of the spleen of a red colour. They appear to arise from the outside of the cells going off at right angles to their circumference, like radii. Where the injection has not been very minute, they are seen to arise at so many points of the capsule; but where the injection has got into smaller branches, their number is so much increased that they appear to form plexuses round the cells.

The trunk of the splenic vein, compared with that of the artery, when both are filled with wax, is found to be in the proportion of five to one in its size. This was ascertained both by an accurate measurement of their diameters, and by weighing half an inch in length of each in a very nice balance; the disproportion between them is greater, than between corresponding veins and arteries, in other parts of the body.

Having acquired this knowledge of the internal structure of the spleen, I made the following experiment with a decoction of madder. This substance was employed, from the animals who feed on it having their bones tinged red, so that there can be no doubt of its colouring matter being carried

into the circulation of the blood. I was much disappointed on seeing the colour of the decoction, which, instead of being a bright red (the tinge communicated to the bones), was of a dirty brown. The same gentlemen assisted me, as in the former experiment.

Nov. 8, 1807, seven ounces of a strong decoction of madder were injected into the stomach of a dog, immediately after the pylorus had been secured. At this time the dog voided some urine, which was limpid and colourless. In 42 minutes, 2 ounces of a yellowish fluid were brought up by vomiting. In 18 minutes more the dog vomited again; what came up proved to consist of $3\frac{1}{2}$ ounces of solid matter, and 3 ounces of liquid. In 15 minutes afterwards, 5 ounces of the decoction were injected, which remained quietly on the stomach for two hours and a quarter, at the end of which period the dog was killed. In the act of dying he made water, in the quantity of two ounces, of a dark muddy colour. This was saved, and afterwards compared with the remaining liquid in the stomach, which it exactly resembled. On examining the connections between the stomach and spleen, none of the absorbent vessels were apparent, more than in the former experiment. The pyloric portion of the stomach contained about two ounces of half digested food, but no liquid. The cardiac portion contained four ounces of liquid, and half an ounce of solid food, so that the act of vomiting, which appeared, at the time, a sufficient exertion to have completely emptied the stomach, had brought up no part of the contents of the pyloric portion, and had not even completely emptied the cardiac portion. In this experiment, without making allowance for any liquid in the stomach, prior to the decoction of madder being injected, one-fourth part of the quantity thrown in had escaped.

The cells of the spleen were more distinctly seen than in the former experiment, particularly at the great end.

Although there was every reason to believe that the colouring matter of the madder had been conveyed into the urinary bladder, yet so muddy and indistinct was the colour, that it was by no means completely ascertained. I therefore resolved in my future experiments, to make use of some colouring substance, the presence of which could be detected in a very diluted state, by means of a chemical test; and I requested Mr. W. Brande, of whose assistance I have before availed myself, to point out the substances best fitted for this purpose. He immediately suggested that rhubarb was a substance which he had made use of as a test to ascertain the presence of alkali, and therefore had no doubt that the caustic alkali would prove a test of rhubarb. This substance has also another advantage; it is well known to pass very readily by the kidneys, without being decomposed.

The following are the results of experiments made with rhubarb, to ascertain the best modes of detecting it in the urine and blood, and the time it takes to pass from the stomach to the urinary bladder.

Five drops of tincture of rhubarb added to 8 ounces of water, are found to strike an orange tint when the test is added, which does not take place when the rhubarb is more diluted.

Six drops of tincture of rhubarb, added to three ounces of serum, are readily detected by the eye, but the colour is not heightened by applying the test; the alkali contained in the serum, being sufficient to strike as bright a tint, as that quantity of rhubarb can receive from the addition of alkali.

When tincture of rhubarb is mixed with blood just taken from the arm, its colouring matter is afterwards found both in the serum and in the coagulum.

When blood is drawn from the arm of a person, who has taken rhubarb in sufficient quantity to affect the urine, the serum is found to have a slight tinge from it, equal to that, which one drop of tincture of rhubarb gives to half an ounce of serum when added to it.

Half an ounce of tincture of rhubarb, diluted in $1\frac{1}{2}$ ounce of water, taken in the interval between meals, did not pass off by urine in less than an hour, and even then was not in sufficient quantity to be discovered, till the test was applied.

The same quantity was taken immediately before a breakfast consisting of tea. In 17 minutes, half an ounce of urine was voided, which when tested had a light tinge. In 30 minutes another half ounce was made, in which the tinge was stronger; and in 41 minutes a third half ounce was made, in which it was very deep. In an hour and ten minutes 7 ounces were voided, in which the tinge of rhubarb was very weak, and in two hours twelve ounces were voided, in which it was hardly perceptible.

In $6\frac{1}{2}$ hours the rhubarb acted on the bowels, and gave a decided tinge to the fæces; the urine made at the same time had a much stronger tinge, than what was voided at one hour and ten minutes.

In this experiment, the rhubarb appeared to have escaped from the cardiac portion of the stomach; and in two hours ceased to pass through that channel; but was afterwards carried into the system from the intestines, and again appeared in the urine.

This experiment was repeated on another person; the rhubarb was detected in the urine in 20 minutes. In 2 hours the tinge became very faint; in 5 hours it was scarcely perceptible; in seven hours the rhubarb acted on the bowels; and the urine made after that period, became again as highly tinged as at first.

It was suggested by a chemical friend, that the prussiate of potash might be a better substance than rhubarb, for the present experiments, since the solution of one quarter of a grain in two ounces of water, becomes of a blue colour on the addition of the acidulous muriate of iron.

To determine this point, one quarter of a grain was dissolved in two ounces of serum, but no blue colour was produced by the addition of the test, nor did this effect take place till the quantity of the prussiate was encreased to a grain; so that minute quantities of the prussiate of potash, or at least of the prussic acid, may exist in the blood, without being detected by adding solution of iron.

The effects of rhubarb on the urine, and the different parts of the blood having been thus ascertained, a third experiment was made, in which that substance was employed, and I had the assistance of the same gentlemen as in the others.

On November 17, 1807, at 35 minutes past 11 o'clock, five drams of a mixture of tincture of rhubarb and water, in the proportion of a dram to an ounce, were injected into the stomach of a dog, whose pylorus was secured. At 20 minutes past 1, two ounces of fluid were brought up by vomiting; ten minutes afterwards, another ounce of the mixture was injected, as were nine drams more at ½ past 4 o'clock. The two last portions were retained, and at 8 o'clock in the evening the dog was killed.

On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

This urine tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubarb had been taken three hours by the mouth, and in other respects resembled it.

When the spleen was cut into, the cells were particularly large and distinct. A portion of it was then macerated in two drams of water for ten minutes in a glass vial. All the parts were exposed to the water, by its being divided in all directions. The water thus impregnated was strained off and tested by the alkali, and immediately the reddish brown colour was produced in the centre, and no where else, but in less than a minute it began to diffuse itself, and extended over the whole.

A similar portion of the liver was treated in the same way, and the alkali was added to the strained liquor, but no change took place in it whatever.

In this experiment the rhubarb was detected in the juices of the spleen as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there through the medium of the common absorbents carrying it into the thoracic duct, and afterwards into the circulation of the blood.

The discovery of this fact I consider to be of sufficient importance to be announced to the Society, that when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiments, to prosecute the enquiry.

III. *On the Composition of the Compound Sulphuret from Huel Boys, and an Account of its Crystals.* By James Smithson, *Esq. F. R. S.*

Read January 28, 1808.

It is but very lately that I have seen the Philosophical Transactions for 1804, and become acquainted with the two papers on the compound sulphuret of lead, antimony, and copper contained in the first part of it, which circumstance has prevented my offering sooner a few observations on Mr. Hatchett's experiments, which I deem essential towards this substance being rightly considered, and indeed the principles of which extend to other chemical compounds; and also giving an account of the form of this compound sulphuret, as that which has been laid before the Society is very materially inaccurate and imperfect.

We have no real knowledge of the nature of a compound substance till we are acquainted with its proximate elements, or those matters by whose direct or immediate union it is produced; for these only are its true elements. Thus, though we know that vegetable acids consist of oxygene, hydrogene, and carbon, we are not really acquainted with their composition, because these are not their proximate, that is, are not their elements, but are the elements of their elements, or the elements of these. It is evident what would be our acquaintance with sulphate of iron; for example, did we only know that a crystal of it consisted of iron, sulphur, oxygene, and

hydrogene; or of carbonate of lime, if only that it was a compound of lime, carbon or diamond, and oxygene. In fact, totally dissimilar substances may have the same ultimate elements, and even probably in precisely the same proportions; nitrate of ammonia, and hydrate of ammonia, or crystals of caustic volatile alkali,* both ultimately consist of oxygene, hydrogene, and azote.

It is not probable that the present ore is a direct quadruple combination of the three metals and sulphur, that these, in their simple states, are its immediate component parts; it is much more credible that it is a combination of the three sulphurets of these metals.

On this presumption I have made experiments to determine the respective proportions of these sulphurets in it.

I have found 10 grains of galena, or sulphuret of lead, to produce 12.5 grains of sulphate of lead. Hence the 60.1 grains of sulphate lead, which Mr. HATCHETT obtained, correspond to 48.08 grains of sulphuret of lead.

I have found 10 grains of sulphuret of antimony to afford 11,0 grains of precipitate from muriatic acid by water. Hence 31.5 grains of this precipitate are equal to 28.64 grains of sulphuret of antimony.

The want of sulphuret of copper has prevented my determining the relation between it and black oxide of copper, but this omission is, it is evident, immaterial, as the quantity of this sulphuret in the ore must be the complement of the sum of the two others.

But as the iron is a foreign adventitious substance in this ore, it follows that the foregoing quantities are the products

* FOURCROY, *Syst. des Con. Chem.* t. I. p. 88.

of only 96.65 grains of it. 100 parts of the ore are therefore composed of

Sulphuret of lead -	49.7
Sulphuret of antimony	29.6
Sulphuret of copper -	20.7
	100.0

It is impossible not to be struck with the trifling alteration which these quantities require to reduce them to very simple proportions, or to think it a very great violation of probability to suppose that experiments, effected with no errors, would have given them thus:

Sulphuret of lead -	50.
Sulphuret of antimony -	30.
Sulphuret of copper -	20.

However, I doubt the existence of triple, quadruple, &c. compounds; I believe, that *all combination is binary;* that no substance whatever has more than two proximate or true elements; and hence I should be inclined to consider the present compound as a combination of galena and fahlertz; and if so, it will be accurately represented, as far as chemical analysis has yet been able to go, by the following figure:

Compound sulphuret of lead, antimony, and copper = { ½ galena = { ⅓ sulphur, ⅔ lead ; ½ fahlertz = { ⅓ sulphuret of antimony = { ⅕ sulphur, ⅘ antimony ; ⅔ sulphuret of copper = { ⅖ sulphur, ⅗ copper.

Its ultimate elements are therefore,

Sulphur -	20 . . .	= 19/60
Lead -	41⅔ . . .	= 25/60

Antimony - 25 . . . . = $\frac{1}{60}$

Copper - $13\frac{1}{3}$ . . . = $\frac{8}{60}$

and it is not a little remarkable, that here, as was the case with the calamine,* they are sexagesimal fractions of it.

When in a former paper I offered a system on the proportions of the elements of compounds, I supported it by the results of my own experiments, which might be supposed influenced, even unconsciously to myself, by a favourite hypothesis, and I made the application of it principally to a substance whose nature was not very clear. But the present case is not liable to these objections: here no fondness to the theory can be suspected of having led astray, nor did even the experiments as they came from their author's hands, bear an appearance in the least favourable to it, and yet when properly considered, they are found to accord no less remarkably with its principles.

It is evident that there must be a precise quantity in which the elements of compounds are united together in them, otherwise a matter, which was not a simple one, would be liable, in its several masses, to vary from itself, according as one or other of its ingredients chanced to predominate; but chemical experiments are unavoidably attended with too many sources of fallacy for this precise quantity to be discovered by them; it is therefore to theory that we must owe the knowledge of it. For this purpose an hypothesis must be made, and its justness tried by a strict comparison with facts. If they are found at variance, the assumed hypothesis must be relinquished with candour as erroneous, but should it, on the contrary prove, on a multitude of trials, invariably to accord with

* Phil. Trans. 1803, p. 12.

the results of observation, as nearly as our means of determination authorise us to expect, we are warranted in believing that the principle of nature is obtained, as we then have all the proofs of its being so, which men can have of the justness of their theories: a constant and perfect agreement with the phenomena, as far as can be discovered.

The great criterion in the present case is, whether on the conversion of a substance into its several compounds, and of these into one another, the simple ratios always obtain which the principles of the theory require. Amongst the multitude of instances which I could adduce, in support of such being the fact, I will, for the sake of brevity, confine myself to a few in the substances which have come under consideration above, as they will likewise give the grounds on which some of the proportions in the table have been assigned, and every chemist, by a careful repetition of the experiments, may easily determine for himself to what attention the present theory is entitled.

Lead - -	= $\frac{1}{2}$ of sulphate of lead
	= $\frac{6}{7}$ of sulphuret of lead
Sulphuret of lead	= $\frac{1}{2}$ of lead
	= $\frac{6}{7}$ of sulphate of lead
Sulphate of lead	= $\frac{1}{3}$ of lead
	= $\frac{6}{7}$ of sulphuret of lead
Antimony - -	= $\frac{1}{3}$ of powder of algoroth
	= $\frac{6}{5}$ of sulphuret of antimony
Sulphuret of antimony -	= $\frac{10}{9}$ of powder of algoroth.

In the experiments by which these relations were ascertained, the portion of powder of algoroth and sulphate of lead

dissolved in the precipitating and washing waters, was scrupulously collected.

The importance of a knowledge of the true quantity in which matters combine, is too evident to require to be dwelt upon; but this importance will be greatly augmented, if it should prove that this quantity is, as has been suggested, expressive of the forces with which they attract each other. It is perhaps in the form of matters that we shall find the cause of the proportions in which they unite, and a proof, *a priori*, of the system here maintained.

I have examined some of the grey ores of copper in tetraedral crystals; but the notes of my experiments are in England. I can, however, say, that they do contain antimony, and that they do not contain iron in any material quantity. With respect to the proportions of the constituent parts, I cannot now speak with any certainty; but, I think, that at least some species of fahlertz contain a smaller portion of sulphuret of antimony, than the fahlertz does which exists as an element in the foregoing compound one.

Of the Form of this Substance.

Of the seventeen figures which have been given, as of the crystals of this compound sulphuret, in Part II. of the volume of the Transactions for 1804, great part are acknowledged to have no existence, nor are indeed any of them consistent with nature.

This substance seems to have yet offered but one form, and

which is represented in the annexed Plate under its two principal appearances; that is, having the primitive faces, the predominant ones of the prism; and having the secondary ones such, and which will be fully sufficient to make it known. In the first infancy of the study of crystals, it might be necessary to attend to every, the most trifling, variation of them, to trace each of their changes, step by step, to, as it were, spell the subject; but in the state to which the science has now attained, to continue to do so would be not only superfluous, but most truly puerile.

I have a very small, but very regular, crystal of the form of Fig. 1.

By mensuration the faces *a* and *m* appear to form together an angle of about 135°, and the faces *c* and *b* an angle of about 125°.

It is said in the account above quoted, that the primitive form of this matter is a rectangular tetraedral prism, but no proofs of this have been offered; nor have the dimensions of this prism been given, a circumstance of the first moment to the determination of true or primitive form, nor have any quantities been assigned to the decrements supposed. I will, therefore, supply these very important omissions.

That the atom of this substance is a rectangular tetraedral prism, is inferable, not from the striæ on the crystals, for striæ are by no means invariably indicative of a decrement in the direction of them; but from the angles which the faces *a* and *c* make with the faces *m* and *b*, and these angles also prove, that the height of this prism is equal to the side of its base, that is, that it is a cube.

Hence the face *a* is produced by a decrease of one row of

atoms along the edge of the cube, and the angle it forms with the face *m* is really of 135°.

The face *c* is produced by a decrease of two rows of atoms at the corners of the cube, and the angle it forms with the face *b* is = 125° 15′ 52″.

The face *b* being produced like the face *a*, forms the same angle with the face *m*.

No crystal I possess, has enabled me to measure the inclinations of the faces *g*, *d*, or *f*; should the face *g*, as is presumable, result from a decrease of one row of atoms at the corners of the cube, it will form with the face *b*, an angle of 144° 44′ 8″, and if the faces *d* and *f* are, as is also probable, produced by a decrease of two rows of atoms along the edges of the cube, the first will form an angle of 116° 33′ 54″, and the latter one of 153° 26′ 6″, with the face *m*.

The angles assigned here differ considerably from those given in the former account of these crystals; but the angles there given have not only appeared to me to be contradicted by observation, but, crystallographically considered, are inconsistent with each other, as the tetraedral prism of dimensions to produce an angle of 135° by a decrement along its edge, would not afford angles of 140° and 120° by decrements at its corners.

The sum of the faces of these crystals is 50.

Fig. 1.

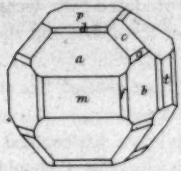
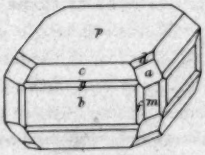


Fig. 2.



m p	=	*90°*		
m t	=	*90°*		
a m	=	*135°*		
m b	=	*135°*		
c b	=	*125°*	*15′*	*52″*
g b	=	*144°*	*44′*	*8″*
d m	=	*116°*	*33′*	*54′*
f m	=	*153°*	*26′*	*6″*

IV. *On Oxalic Acid.* *By* Thomas Thomson, *M.D. F.R.S. Ed.* *Communicated by* Charles Hatchett, *Esq. F. R. S.*

Read January 14th, 1808.

Oxalic acid, from the united testimony of Ehrhart, Hermbstadt, and Westrumb, appears to have been discovered by Scheele; but it is to Bergman that we are indebted for the first account of its properties. He published his dissertation on it in 1776, and since that time very little has been added to the facts contained in his valuable treatise. Chemists have chiefly directed their attention to the formation of that acid, and much curious and important information has resulted from the experiments of Hermbstadt, Westrumb, Berthollet, Fourcroy, and Vauquelin, &c. but the properties of the acid itself, have been rather neglected. My object in the following pages is not to give a complete history of the properties of oxalic acid, but merely to state the result of a set of experiments, undertaken with the view of ascertaining different particulars respecting it, which I conceived to be of importance.

1. *Water of Crystallization.*

Oxalic acid is usually obtained in transparent prismatic crystals more or less regular; these crystals contain a portion of water, for when moderately heated they effloresce

and lose a part of their weight, which they afterwards recover when left exposed in a moist place. When cautiously heated on a sand bath they fall to powder, and lose about a third of their weight. But as the acid is itself volatile, it is not probable that the whole of this loss is water. To ascertain the quantity of water contained in these crystals I had recourse to the following method.

1. Seventy grains of crystallized oxalic acid were dissolved in 600 grains of water, constituting a solution which weighed 670 grains.

Fifty grains of pure carbonate of lime, in the state of calcareous spar, were dissolved in muriatic acid; this solution was evaporated to dryness to get rid of the excess of acid, and the residue redissolved in water.

Into this muriate of lime the solution of oxalic acid was dropt by little and little as long as any precipitate fell, and the oxalate of lime thus formed was separated by the filter. Pure oxalic acid is not capable of precipitating the whole lime from solution of muriate of lime, the muriatic acid evolved being always sufficient to retain the last portions in solution.

It was necessary to get rid of this excess of acid; the method which appeared the least exceptionable was to saturate the muriatic acid with ammonia; accordingly when the oxalic acid ceased to occasion any farther precipitate, I cautiously added pure ammonia, till the liquid ceased to produce any effect upon vegetable blues. A copious additional precipitate of oxalate of lime was thus obtained. Oxalic acid was now added again as long as it rendered the liquid muddy. By thus alternately having recourse to the acid solution, and to ammonia, and by adding both with great caution to avoid any excess, I

succeeded in separating the whole of the lime without using any sensible excess of oxalic acid.

558 grains of the acid solution were employed, a quantity which is equivalent to 58.3 grains of the crystallized acid.

2. The oxalate of lime, after being well washed and drained, and exposed for a week to the open air, at a temperature of about 60°, weighed 76 grains; but upon being left on the sand bath for some hours in a temperature between 200° and 300°, its weight was reduced to 72 grains.

3. These 72 grains of dry oxalate of lime were put into an open platinum crucible, and gradually heated to redness. By these means they were reduced to 49.5 grains, which proved to be carbonate of lime. The crucible was now exposed to a violent heat in a forge. Nothing remained but a quantity of pure lime weighing 27 grains.

4. From this experiment we learn, that 72 grains of dry oxalate of lime contain 27 grains of lime. Of consequence, the oxalic acid in this compound must be 45 grains. But the weight of crystallized oxalic acid actually used was 58.3 grains, a quantity which exceeds the whole acid in the oxalate by 13.3 grains. These 13.3 grains are the amount of the water of crystallization, which either did not unite with the salt, or was driven off by the subsequent exposure to heat. Hence crystallized oxalic acid is composed of

Real acid - -	45.
Water - -	13.3
	58.3

Now this is equivalent to

Real acid - -	77
Water - -	23
	100

So that the crystals of oxalic acid contain very nearly the fourth part of their weight of water.*

II. *Alkaline and Earthy Oxalates.*

1. The preceding experiment gives us likewise the composition of oxalate of lime. This salt, when merely dried in the open air, still retains a portion of water which may be driven off by artificial heat. It is necessary to know that it parts with this water with considerable difficulty, so that a long exposure on the sand or steam bath, is necessary to get it thoroughly dry. It afterwards imbibes a little water if it be left in a moist place. Well dried oxalate we have seen is a compound of

Acid	45	or per cent, 62.5 acid.
Base	27	- - 37.5 base.
	72	100

* VAUQUELIN in a late dissertation on cinchona, marked with that profound skill which characterizes all the productions of this illustrious chemist, has mentioned incidentally, that the crystals of oxalic acid contain about half their weight of water. He dissolved 100 parts of cinchonate of lime in water, and precipitated by means of oxalic acid; 22 parts of crystallized oxalic acid were necessary; and the oxalate of lime formed weighed 27 grains. From this experiment he draws the conclusion which I have stated, (See Ann de Chem. lix. 164.) But this ingenious chemist does not seem to have been aware of the real composition of oxalate of lime. 27 grains of that salt are composed very nearly of 10 grains of lime and 17 grains of acid. But the weight of the crystals used by VAUQUELIN was 22; the difference, 5, is obviously the water of crystallization in 22 grains of the crystals. But if 22 grains contain 5 of water, it is obvious, that 100 contain very nearly 23. So that his experiment in reality coincides with mine.

Though the oxalate of lime dried spontaneously can scarcely be considered as always in the same state, yet as the difference in the portion of water which it retains is not great, provided it be dried slowly in the temperature of 60°, and in a dry place, it may be worth while to state its composition. It is as follows:

Acid	45 or per cent	59.2 acid.
Base	27	35.5 base.
Water	4	5.3 water.
	76	100.0

When rapidly dried, as by pressing it between the folds of filtering paper, it is apt to concrete into hard lumps, which retain more moisture. In this state I have sometimes seen it retain 10 per cent. of water after it appeared dry.

Bergmann states the composition of oxalate of lime as follows:

Acid -	48
Lime - -	46
Water - -	6
	100*

His method was to dissolve a determinate quantity of calcareous spar in nitric acid, and then to precipitate the lime by oxalic acid. 100 parts of calcareous spar thus dissolved, require, according to him, 85 parts of crystallized acid to precipitate them. But there must have been some mistake in this experiment; for, according to my trials (provided the nitric acid be carefully neutralized by ammonia as it is evolved), no

* Opusc. I. 262.

less than 117 grains of oxalic acid would have been required, and at least 145 grains of oxalate of lime would have been obtained instead of the 119, which was the result of BERGMANN's experiment. It is obvious that BERGMANN did not precipitate all the lime. He added oxalic acid till it ceased to produce any effect on the solution from the great excess of nitric acid evolved; and then took it for granted that all the lime was separated. But had he added ammonia, he would have got an additional quantity of oxalate of lime, and the precipitation would have recommenced upon adding more oxalic acid. This explanation accounts in a satisfactory manner for the difference between BERGMANN's statement of the composition of oxalate of lime, and mine.

2. Though the preceding experiment was made with care, yet as some of the most important of the following observations in some measure rest upon the analysis of oxalate of lime, I thought it worth while to verify that analysis in the following manner.

100 grains of crystallized oxalic acid were dissolved in 1000 grains of water, making a solution which weighed 1100 grains.

It is obvious that every 100 grains of the above solution contained 9.09 grains of crystal of oxalic acid, equivalent, according to the preceeding analysis, to 7 grains of real acid.

100 grains of this solution were gradually mixed with lime water till the liquid ceased to produce any change on vegetable blues. The oxalate of lime thus formed being well dried, weighed 11.2 grains. Exposed to a violent heat in a platinum crucible, this salt left 4.2 grains of pure lime. Hence it was composed of

7	acid,	or per cent.	62.5 acid
4.2	lime		37.5 base
11.2			100.0

Thus we have obtained exactly the same result as in the former experiment, both as far as relates to the composition of oxalate of lime, and likewise to the proportion of water of crystallization in crystallized oxalic acid.

The lime water necessary to saturate the acid amounted to 3186 grains. Hence, it contained only $\frac{1}{759}$th of its weight of lime.

3. The oxalates of barytes and strontian are white, tasteless powders, which may be obtained by mixing oxalate of ammonia with the muriates of these alkaline earths. It is said that these earths are capable of forming soluble superoxalates with this acid; but I have not tried the experiment. These oxalates, as well as oxalate of lime, are partially soluble in the strong acids.

4. Oxalate of magnesia is a soft white powder, bearing a considerable resemblance to oxalate of lime. It is tasteless, and not sensibly soluble in water; yet when oxalate of ammonia is mixed with sulphate of magnesia, no precipitate falls; but if the solution be heated and concentrated sufficiently, or if it be evaporated to dryness, and redissolved in water, in both cases the oxalate of magnesia separates in the state of an insoluble powder.

5. Oxalate of potash readily crystallizes in flat rhomboids, commonly terminated by dihedral summits. The lateral edges of the prism are usually bevelled. The taste of this salt is cooling and bitter. At the temperature of 60° it dissolves in thrice its weight of water. When dried on the sand bath, and afterwards

exposed in a damp place, it absorbs a little moisture from the atmosphere.

This salt combines with an excess of acid, and forms a superoxalate, long known by the name of *salt of sorrel*. It is very sparingly soluble in water, though more so than tartar. It occurs in commerce in beautiful 4-sided prisms attached to each other. The acid contained in this salt is very nearly double of what is contained in oxalate of potash. Suppose 100 parts of potash; if the weight of acid necessary to convert this quantity into oxalate be x then $2x$, will convert it into superoxalate.

6. Oxalate of soda readily crystallizes. Its taste is nearly the same as that of oxalate of potash. When heated, it falls to powder, and loses the whole of its water of crystallization. Soda is said to be capable of combining with an excess of acid, and of forming a superoxalate. I have not tried the experiment.

7. Oxalate of ammonia is the most important of all the oxalates, being very much employed by chemists to detect the presence of lime, and to separate it from solutions. It crystallizes in long transparent prisms, rhomboidal, and terminated, by dihedral summits. The lateral edges are often truncated, so as to make the prism 6 or 8-sided. Sometimes the original faces of the prism are nearly effaced.

The taste of this salt is bitter and unpleasant, somewhat like that of sal ammoniac. At the temperature of 60°, 1000 grains of water dissolve only 45 grains of this salt. Hence, 1000 grains of saturated solution of oxalate of ammonia contain only 43.2 grains of this salt. The specific gravity of this solution is 1.0186. As it may be useful to know the weight

of this salt contained in solutions of different specific gravities, I have thought it worth while to construct the following table:

Weight of oxalate of ammonia in 100 parts of the solution.	Specific gravity of the solution at 60°.	Weight of oxalate of ammonia in 100 parts of the solution.	Specific gravity of the solution at 60°.
4.32	1.0186	1.5	1.0075
4.	1.0179	1.	1.0054
3.5	1.0160	0.5	1.0030
3.	1.0142	0.4	1.0024
2.5	1.0120	0.3	1.0018
2.	0.0095	0.2	1.0012
		0.1	1.0006

8. To determine the composition of these salts, I took seven different portions of a diluted oxalic acid solution, each weighing 100 grains, and containing 7 grains of real oxalic acid. To each of these portions I added respectively potash, soda, ammonia, barytes water, strontian water, and lime water, till it ceased to produce any change. The liquid was then evaporated to dryness, and the residue, after being well dried on the steam bath, was weighed. Each of these salts contained 7 grains of acid; the additional weight I ascribed to the base. Hence I had the following table, which exhibits the weight of each salt obtained, and its composition deduced from that weight.

Salts.	Weight obtained.	Composition.	
		Acid.	Base.
Oxalate of Ammonia	9.4	7	2.4
——— Magnesia*	9.5	7	2.5
——— Soda -	11.0	7	4.0
——— Lime -	11.2	7	4.2
——— Potash -	15.6	7	8.6
——— Strontian	17.6	7	10.6
——— Barytes -	17.0	7	10.0

The composition of these salts reduced to 100 parts, is given in the following table.

	Oxalate of Ammonia.	Ox. of Magnesia.	Ox. of Soda.	Ox. of Lime.	Ox. of Potash.	Ox. of Strontian.	Ox. of Barytes.
Acid	74.45	73.68	63.63	62.50	44.87	39.77	41.16
Base	25.53	26.32	36.37	37.50	55.13	60.23	58.84
Total	100.	100.	100.	100	100	100	100

* The oxalate of magnesia was obtained by neutralizing the oxalic acid solution with ammonia, then mixing it with sulphate of magnesia, evaporating the solution to dryness, and washing the insoluble oxalate of magnesia with a sufficient quantity of water.

But for practical purposes, it is more convenient to consider the acid as a constant quantity. The following table is constructed upon that plan.

	Acid.	Base.	Weight of Salt.
Oxalate of Ammonia	100	34.12	134.12
—— Magnesia	100	35.71	135.71
—— Soda	100	57.14	157.14
—— Lime	100	60.00	160.00
—— Potash	100	122.86	222.86
—— Strontian	100	151.51	251.51
—— Barytes	100	142.86	242.86

9. In the preceding statement, no account has been taken of the water of crystallization which might still remain attached to the salts, notwithstanding the heat to which they were exposed. There is reason to believe, however, that in most of them this water must be so small, that it may be overlooked without any great error. Oxalates of soda and of ammonia, I have reason to believe, lose all their water of crystallization at a moderate heat. This is the case also with oxalates of lime and barytes, and I presume that the oxalates of strontian and magnesia are not exceptions; but oxalate of potash retains its water much more obstinately. I believe that in that salt the weight of acid and of base are nearly equal, and that when dried in the temperature of 212°, it still retains nearly 10 per cent. of water; but I have not been able to establish this opinion by direct experiment.

The composition of oxalate of strontian in the preceding table, was so different from what I expected, that I repeated

the experiment; but the result was the same. This induced me to combine strontian and oxalic acid in the following manner: 100 grains of a solution containing 7 grains of real oxalic acid were neutralized by ammonia, and the oxalic acid precipitated by means of muriate of strontian. The salt obtained weighed 12.3 grains; of course it was composed of

Acid	7	or	56.9	or	100
Base	5.3		43.1		75.7
	12.3		100.0		175.7

Thus it appears that there are two oxalates of strontian, the first obtained by saturating oxalic acid with strontian water, the second by mixing together oxalate of ammonia and muriate of strontian. It is remarkable that the first contains just double the proportion of base contained in the second.

III. *Decomposition of the Oxalates.*

1. When oxalic acid, in the state of crystals, is exposed to heat, it is only partially acted upon, a considerable portion escaping without alteration; but when an alkaline or earthy oxalate is heated, the acid remains fixed till it undergoes complete decomposition. The new substances into which the acid is converted, as far as my experience goes, are always the same, what oxalate soever we employ. They are five in number; namely, *water*, *carbonic acid*, *carbonic oxide*, *carbureted hydrogen*, and *charcoal*.

2. The water is never quite pure. Though no sensible portion of oil can be perceived in it, yet it has always the peculiar smell of the water obtained during the distillation of wood; a smell which is usually ascribed to oil. It commonly

shews traces of the presence of ammonia, changing vegetable blues to green, and smoking when brought near muriatic acid; but this minute portion of ammonia is probably only accidentally present. All the oxalates which I decomposed by distillation, were obtained by double decomposition from oxalate of ammonia; and though they were washed with sufficient care, yet I think it not unlikely that a minute portion of oxalate of ammonia might continue to adhere. Practical chemists know the extreme difficulty of removing every trace of a salt with which another has been mixed.

The carbonic acid remains partly combined with the base, which always becomes a carbonate, and partly makes its escape in the form of gas.

The carbonic oxide and carbureted hydrogen make their escape in the form of gas: the charcoal remains in the retort mixed with the base, to which it communicates a grey colour: the quantity of it depends in some measure upon the heat. If the oxalate was exposed to a very violent heat, no charcoal at all remains. Hence it probably acts upon the carbonic acid united to the base, converting it into carbonic oxide, as happens when a mixture of a carbonate and charcoal are heated.

3. I was induced to examine this decomposition with considerable attention, because I conceived that it would furnish the means of estimating the composition of oxalic acid; and I pitched upon oxalate of lime, as the salt best adapted for the purpose I had in view. A determinate quantity of this salt was put into a small retort, and gradually heated to redness. This retort was connected with a pneumatic trough by means of a long glass tube, having a valve at its extremity which allowed gas to issue out, but prevented any water from

entering the tube. The experiment was repeated three times.

4. A hundred grains of oxalate of lime, when thus heated, yield above sixty cubic inches of a gas, which is always a mixture of carbonic acid and inflammable air, nearly in the proportion of one part of the former to three and a half of the latter, reckoning by bulk. The specific gravity of the inflammable gas was 0.908, common air being 1.000; it burns with a blue flame, and when mixed with oxygen, may be kindled by the electric spark. The loudness of the report depends upon the proportion of oxygen.

The smallest quantity of oxygen, with which it can be mixed, so as to burn by the electric spark, is 1-9th; the combustion is very feeble, and is attended with no perceptible report. If the residue be washed in lime water and mixed with 1-9th of its bulk of oxygen, it may be kindled a second time: this may be repeated five times, after which the residue cannot be made to burn.

The combustion becomes more violent, and the report louder, as we increase the proportion of oxygen, and both are greatest when the oxygen is double the bulk of the gas. As we increase the dose of oxygen, the combustion becomes more and more feeble; and five parts of oxygen and one of gas is the limit of combustion on this side: for a mixture of six parts of oxygen and one of the inflammable air will not burn.

In these experiments the results differ materially from each other, when the proportion of oxygen used is small and when it is great. I am not able at present to account for this difference, which holds not only with respect to this gas, but

every compound inflammable gas which I have examined. This difference makes it impossible to use both extremes of the series: I make choice of that in which the proportion of oxygen is considerable, as upon the whole more satisfactory. The best proportion is one part of the gas and two parts of oxygen. The oxygen ought not to be pure, but diluted with at least the third of its bulk of azote, unless the gas be much contaminated with common air.

I have elsewhere detailed the method which I follow in analyzing gases of this nature.* The following table exhibits the mean of a considerable number of trials of this gas with oxygen.

Measures of inflammable Air consumed.	Measures of Oxygen consumed.	Carbonic Acid formed.	Diminution of Bulk.
100	91	93	98

that is to say, 100 cubic inches of the gas when burnt, combine with 91 cubic inches of oxygen; there are produced 93 inches of carbonic acid; and after the combustion these 93 inches alone remain, the rest being condensed. Hence we conclude that the other substance produced was water.

This result corresponds almost exactly with what would have been obtained, if we had made the same experiment upon a mixture of 70 measures of carbonic oxide, and 30 measures of carbureted hydrogen, as will appear from the following table.

* See NICHOLSON'S Journal, 16. 247.

	Measures of inflammable Gas consumed.	Measures of Oxygen consumed.	Measures of carbonic Acid formed.	Diminution of Bulk.
Carbonic oxide	70	31.5	63	38.5
Carbureted hydrogen	30	60.0	30	60.0
Total	100	91.5	93	98.5

This coincidence is so exact, that I do not hesitate to conclude that the inflammable gas, which was the subject of experiment, was in reality a mixture of 70 parts of carbonic oxide, and 30 of carbureted hydrogen. The specific gravity indeed, which was 0.908, does not exactly agree with the specific gravity of such a mixture; for 2⅓ measures of carbonic oxide, and one measure of carbureted hydrogen, ought to form a mixture of the specific gravity 0.849, provided the specific gravity of carbonic oxide be 0.956, and that of carbureted hydrogen 0.600; but this objection cannot be admitted to be of much weight, till the specific gravity of pure carbureted hydrogen be ascertained with more accuracy than has hitherto been done.

The results contained in the preceding table, enable us to determine the composition of this inflammable air with considerable precision; for 100 cubic inches of it require 91 inches of oxygen, and form 93 cubic inches of carbonic acid. But it is known that carbonic acid gas requires for its formation a quantity of oxygen gas equal to its own bulk: therefore to form 93 inches of it, 93 inches of oxygen gas must have been employed; but only 91 were mixed with the gas: therefore the

gas itself must have furnished a quantity of oxygen, equivalent to the bulk of two cubic inches, besides all the carbon contained in 93 inches of carbonic acid.

This carbon amounts in weight to	12.09 grains.
Two cubic inches of oxygen weigh	.68
Total	12.77

But as 100 cubic inches of the gas weigh 28.15 grains, it is obvious that besides the 12.77 grains which it furnished to the carbonic acid, it must have contained 15.38 grains of additional matter; but as the only two products were carbonic acid and water, it is plain that the whole of this additional matter must, by the explosion, have been converted into water. Its constituents of course must have been

13.19 oxygen
2.19 hydrogen

15.38

Adding this to the 12.77 grains formerly obtained, we get the composition of the gas as follows:

Oxygen	13.87
Carbon	12.09
Hydrogen	2.19
	28.15

which reduced to 100 parts, becomes

Oxygen	49.27
Carbon	42.95
Hydrogen	7.78
	100.00.

5. The residue which remained in the retort, after the distillation was over, was a grey powder, not unlike pounded clay slate. To ascertain its constituents, it was dissolved in diluted nitric acid with the necessary precautions; the loss of weight indicated the quantity of carbonic acid. The charcoal remaining undissolved, was allowed to subside, carefully washed by repeated affusions of water, and then dried in a glass or porcelain capsule. It must not be separated by the filter, for it adheres so obstinately that it cannot be taken off the paper, nor weighed. The nitric acid solution was precipitated by carbonate of soda, and the carbonate of lime obtained was violently heated in a platinum crucible. What remained was pure lime.

6. I shall now detail one of my experiments more particularly. Eighty-nine grains of well dried oxalate of lime were exposed in a small retort to a heat gradually raised to redness; the products were the following:

	Grains.
45.6 cubic inches of gas* weighing	14.8
Water - - -	6.4
Residue in retort - - - -	62.4
	83.6
Loss - - -	5.4
Total	89.0

The loss is obviously owing to the gas which filled the retort and tube when the experiment was concluded. We are

* The gas obtained measured 60 cubic inches, but 14.4 inches of these were found to be common air which had previously filled the retort and tube; this quantity was therefore deducted.

warranted therefore to add it to the weight of the gaseous products obtained.

Now the gas was composed of

Carbonic acid 10.5 cubic inches = 4.9 grains.
Inflammable air 35.1 - - = 9.9

so that one-third of the weight was carbonic acid, and two-thirds inflammable air. If we divide the 5.4 grains of loss, in that proportion we obtain 1.8 grains carbonic acid, and 3.6 grains of inflammable air. Adding these quantities to the weight obtained, we get for the weight of the whole gaseous product

	Grains.
Carbonic acid	6.7
Inflammable air	13.5
	20.2

The 62.4 grains of residue in the retort were composed of

	Grains.
Lime - -	33.4
Carbonic acid	26.4
Charcoal -	2.6
	62.4

Now it is clear, that the 89 grains of oxalate of lime were composed of

Lime -	33.4
Acid - -	55.6
	89.0

The acid was completely decomposed and resolved into the following products:

Carbonic acid	33.1
Inflammable air	13.5
Water -	6.4
Charcoal - -	2.6
	55.6

Had the experiment been made upon 100 grains of oxalic acid instead of 55.6, it is clear that the proportions would have been as follows.

Carbonic acid	59.53
Inflammable air	24.28
Water -	11.51
Charcoal -	4.68
	100.00

The most remarkable circumstance attending the decomposition of oxalic acid by heat, is the great proportion of carbonic acid formed; the quantity amounts to 6-10ths of the whole weight of acid decomposed.

As the composition of all these products of oxalic acid is known with considerable accuracy, it is obvious that they furnish us with the means of ascertaining the constituents of that acid itself.

59.53 grains of carbonic acid are composed of

	Grains.
Oxygen	42.86
Carbon	16.67
	59.53

24.28 grains of inflammable air, according to the analysis given in a preceding part of this paper, are composed of

	Grains.
Oxygen -	11.96
Carbon -	10.48
Hydrogen -	1.89
	24.28

11.51 grains of water are composed of

Oxygen	9.87
Hydrogen	1.64
	11.51

As for the charcoal, though it probably contains both oxygen and hydrogen as well as carbon, yet as the proportion of the two first ingredients is probably very small, and as we have no means of estimating them, we must at present rest satisfied with considering it as composed of pure carbon.

When these different elements are collected under their proper heads, we obtain

1. Oxygen in carbonic acid -	42.86
— — inflammable air -	11.96
— — water -	9.87
	64.69
2. Carbon in carbonic acid -	16.67
— — inflammable air -	10.48
— — charcoal -	4.68
	31.78
3. Hydrogen in inflammable air -	1.89
— — — water -	1.64
	3.53

Hence oxalic acid is composed of oxygen	64.69
— — — — — — — carbon	31.78
— — — — — — — hydrogen	3.53
	100.00

7. The result of two other experiments on oxalate of lime was very nearly the same as the preceding. The following may be stated in round numbers as the mean of the whole. Oxalic acid is a compound of

Oxygen	64
Carbon	32
Hydrogen	4
	100

8. The only other analysis of oxalic acid with which I am acquainted has been given by M. FOURCROY, as the result of his own experiments, in conjunction with those of VAUQUELIN.* It is as follows:

Oxygen	77
Carbon	13
Hydrogen	10
	100

It gave me considerable uneasiness to observe, that my experiments led to conclusions irreconcileable with those of chemists of such eminence and consummate skill, and it was not without considerable hesitation, that I ventured to place any reliance upon them. I am persuaded, however, that some mistake has inadvertently insinuated itself into their calculations; since the carbonic acid alone, formed during the distillation of

* *Systeme de Connoiss Chem.* VII. 224.

oxalate of lime, contains considerably more carbon than the whole quantity which they assign to the oxalic acid decomposed. M. Fourcroy informs us, that oxalic acid is converted into carbonic acid and water, when acted upon by hot nitric acid; and this decomposition seems to have been the method employed, to ascertain the proportion of the constituents of oxalic acid; but the numbers assigned by him do not correspond with this statement. For 10 parts of hydrogen require 60 of oxygen to convert them into water, and 13 of carbon require at least 33 of oxygen. So that instead of 77 parts of oxygen, there would have been required no less than 98 to convert the hydrogen and carbon into water and carbonic acid. It is true, that the surplus of oxygen may be conceived to be furnished by the nitric acid; but if this be admitted (and I have no doubt from experience, that the nitric acid actually does communicate oxygen), it is difficult to see how the constituents of oxalic acid could be determined by any such decomposition, unless the quantity of oxygen furnished by the nitric acid were accurately ascertained.

IV. *Composition of Oxalic Acid.*

The knowledge of the relative weights of the elements which compose oxalic acid, though of importance, is not sufficient to convey a clear idea of this compound, and in what respect it differs from tartaric acid, alcohol, sugar, and various other bodies possessing very different properties, though composed of the very same elements in different proportions.

It has been ascertained, by numerous and decisive experiments, that elementary bodies always enter into combinations in determinate proportions, which may be represented by

numbers. For example, the numbers which correspond to the four elements, oxygen, azote, carbon, and hydrogen, are the following:

Oxygen	6
Azote	5
Carbon	4.5
Hydrogen	1

Now, in all compounds consisting of these ingredients, the proportion of the different constituents may always be represented by these numbers, or by multiples of them: thus, the composition of the following substances may be thus stated.

	Oxygen.	Hydrogen.	Carbon.	Azote.
Water	6	+ 1		
Carbonic oxide	6	- -	+ 4.5	
Carbonic acid	2 × 6	- -	+ 4.5	
Carbureted hydrogen	- -	2 × 1	+ 4.5	
Olefiant gas	- -	1	+ 4.5	
Nitrous gas	6	- -	- -	+ 5
Nitric acid	2 × 6	- -	- -	+ 5
Nitrous oxide	6	- -	- -	+ 2 × 5

From the knowledge of this curious law, it is difficult to avoid concluding that each of these elements consist of atoms of determinate weight, which combine according to certain fixed proportions, and that the numbers above given, represent the relative weights of these atoms respectively. Thus, an atom of oxygen weighs six, an atom of hydrogen one, &c. Water is composed of one atom of oxygen, and one atom of hydrogen: carbonic acid of two atoms of oxygen, and one of carbon, &c. This curious theory, which promises to throw

an unexpected light on the obscurest parts of chemistry, belongs to Mr. Dalton. I have elsewhere illustrated it at considerable length.*

The same law holds with respect to the salts. The acid and bases always combine in determinate proportions. We may affix numbers to all the acids and bases, which numbers, or their multiples, will represent all the combinations into which these bodies enter. Some of these numbers are given in the following table:

Sulphuric acid	38	Barytes	67
Muriatic acid	18	Soda	24
Carbonic acid	16.5	Lime	29
Nitric acid	17	Ammonia	6

These numbers may be conceived to represent the relative weights of an integrant particle of each of these substances: formed on the supposition that an atom of hydrogen weighs 1. It follows equally from this law, that the acids and bases combine particle with particle, or a certain determinate number of particles of the one with a particle of the other.

One of the most important points in the investigation of compound bodies, is to ascertain the number which denotes the weight of an integrant particle of each of them, that of an atom of hydrogen being 1; because this number, or a multiple of it, represents the weight of each, which enters into all combinations; and because it enables us to estimate the number of elementary atoms of which each is composed. From a careful comparison of the table of oxalates, given in a preceding part of this paper, with the weight of the different bases already

* See System of Chemistry, III. 424, &c. 3d Edition.

determined,* it appears that the weight of an integrant particle of oxalic acid must be represented by the number 39.5.

Now, what number of atoms of oxygene, carbon, and hydrogen, go to constitute an integrant particle of oxalic acid? We have assigned the relative weights of each of these atoms, and we have ascertained the relative proportions of the respective elements of oxalic acid. From these data it is easy to solve the problem. An integrant particle of oxalic acid consists of 9 atoms combined together, namely, 4 atoms of oxygen, 3 of carbon, and two of hydrogen.

4 atoms of oxygen weigh	4×6	$= 24$
3 atoms of carbon - -	3×4.5	$= 13.5$
2 atoms of hydrogen -	2×1	$= 2$
Total		39.5

which together make up the weight of an integrant particle of oxalic acid.

According to these proportions, 100 parts of oxalic acid is composed of

Oxygen - -	61
Carbon - -	34
Hydrogen - -	5
	100

numbers which do not indeed exactly correspond with the result of the preceding analysis, but which approach sufficiently near it to give the reasoning employed considerable probability at least, if it does not lead to certainty.

* For these weights, and the method of determining them, I refer the reader to my System of Chemistry, 3d Edition, III. 619. The numbers which I have there assigned are, I am persuaded, rather too low.

We may now examine the decomposition which takes place when oxalate of lime is exposed to heat. Let an atom of oxygen be w, an atom of carbon c, and an atom of hydrogen h. An integrant particle of oxalic acid may be represented by $4w + 3c + 2h$. We may represent the composition and weight of an integrant particle of each of the substances into which oxalic acid is decomposed by heat, by the following symbols and numbers:

Carbonic acid	$2w + c$	weight	16.5
Carbureted hydrogen	$c + 2h$		6.5
Carbonic oxide	$w + c$		10.5
Water	$w + h$		7
Charcoal	c		4.5

We may now conceive 3 particles of oxalic acid to be decomposed at once, and to resolve themselves into these substances, in the following proportions:

4 particles of carbonic acid	$=$	$8w + 4c$
2 particles of carbureted hydrogen	$=$	$2c + 4h$
2 particles of carbonic oxide	$=$	$2w + 2c$
2 particles of water	$=$	$2w + 2h$
1 particle of charcoal	$=$	$1c$
Total		$12w + 9c + 6h$
3 particles of oxalic acid	$=$	$12w + 9c + 6h$

We see that such a decomposition is possible. It remains only therefore to see whether the weights of these substances, which result from this hypothesis, correspond with the preceding analysis. Now,

4 particles of carbonic acid weigh	4 × 16.5	=	66
2 - - carbureted hydrogen	2 × 6.5	=	13
2 - - carbonic acid	2 × 10.5	=	21
2 - - water	2 × 7	=	14
1 - - charcoal	1 × 4.5	=	4.5
Total			118.5

Reducing these proportions to 100 parts of acid, and joining together the two inflammable gases, the numbers come out as follows:

Carbonic acid	55.70	we actually obtained	59.53
Inflammable air	28.69	- -	24.28
Water -	11.81	- - -	11.51
Charcoal -	3.80	- - -	4.68
	100.00		100.00

It is impossible to expect exact correspondence between the theory and hypothesis, till the numbers representing the weights of the elementary atoms be ascertained, with more rigid accuracy than has hitherto been done. I satisfied myself with taking the nearest round numbers, which are sufficient at least to show an evident approximation to the proportions obtained by experiment.

V. *Composition of Sugar, and Formation of Oxalic Acid.*

When a compound body is decomposed, and resolved into a number of new substances, the products are almost always simpler, or consist of integrant particles, composed of fewer atoms than the integrant particles of the original body. Thus, though oxalic acid is composed of 9 atoms, none of the pro-

ducts evolved, when that acid is decomposed by heat, contain more than 3 atoms. Hence it is probable that sugar is a more compound body than oxalic acid, because nitric acid resolves it into a variety of new compounds, one of which is oxalic acid. It may be worth while to examine the action of nitric acid on sugar, and the formation of oxalic acid, more closely than has hitherto been done, as the investigation will furnish some data for estimating the composition of sugar.

Two hundred grains of pure crystallized sugar being treated with diluted nitric acid in the usual way, yielded 200 cubic inches of carbonic acid, 64 cubic inches of nitrous gas, and 70 cubic inches of azotic gas. But these numbers, though the result of a good many experiments, are not to be considered as very exact. The uncertainty depends upon the property which the solution has of producing more gas after the sugar is decomposed, at the expence of the oxalic acid formed. Now, it is difficult to stop at the precise point.

The whole weight of oxalic acid, which can be obtained from 200 grains of sugar, amounts to 116 grains. If the experiment be properly conducted, the whole of the sugar is decomposed, or at least the quantity of residuary matter is small.

From the preceding statement, there is reason to conclude that 100 grains of sugar, when decomposed by nitric acid, yield,

	Grains.
1. Oxalic acid crystals 58 grains, or real acid	45
2. Carbonic acid 100 cubic inches, equivalent to	46.5

while these are evolved obviously by the decomposition of the nitric acid.

	Grains.
1. Azotic gas 35 cubic inches equivalent to	10.62
2. Nitrous gas 32 cubic inches equivalent to	10.85

Now, as nitric acid contains no carbon, it is obvious that the oxalic acid formed, and the carbonic acid evolved, must contain the whole carbon contained in 100 grains of sugar.

	Grains.
45 grains of oxalic acid contain of carbon	14.40
46.5 grains of carbonic acid contain of ditto	13.02
Total	27.42

therefore 100 grains of sugar contain 27½ grains of carbon.

The azotic gas and nitrous gas must have been originally in the state of nitric acid, and must have given out oxygen when they were evolved. But nitric acid is composed of

		Oxygen.
Azote - -	10.62 +	25
Nitrous gas	10.85 +	4.5
		29.5

Therefore they must have parted with 29.5 grains of oxygen. We are at liberty to suppose that the whole of this oxygen went to the formation of carbonic acid. Now, 46.5 grains of carbonic acid are composed of

	Grains.
Oxygen -	33.5
Carbon - -	13.0
	46.5

From this it appears, that in the carbonic acid there were 4 grains of oxygen more than was furnished by the nitric acid. I confess I am disposed to ascribe this surplus to errors

in the experiments, and to believe that the whole of the oxygen of the carbonic acid was furnished by the nitric acid. This being admitted, it follows that the carbon of the carbonic acid, and the whole constituents in the oxalic acid, were furnished by the sugar. These are as follows:

	Grains.
Carbon - - -	27.5
Oxygen in 45 grains oxalic acid	28.8
Hydrogen in ditto - -	1.8
	58.1

If this total be subtracted from the 100 grains of sugar used, there will be a remainder of 41.9 grains. As this quantity of the sugar has disappeared, and is no where to be found among the products, we must suppose that it has assumed the form of water. Now 41.9 grains of water are composed of

Oxygen	35.9
Hydrogen	6
	41.9

Adding these quantities to the preceding products, we obtain the composition of sugar, as follows:

Oxygen	64.7
Carbon	27.5
Hydrogen	7.8
	100.0

Though the process of reasoning, which led to this analysis of sugar, is too hypothetical to be trusted implicitly, yet I am persuaded that it is to a certain degree correct, and that the result obtained does not deviate very far from the truth. If

we compare Lavoisier's statement of the composition of sugar obtained in a different manner, though by a mode of reasoning not less hypothetical, we shall be surprised at its near coincidence with mine. His numbers are

Oxygen	64
Carbon	28
Hydrogen	8
	100

It is true that two different hypotheses may lead to the same result, and yet be both wrong; but this becomes infinitely improbable in the present case, when we consider that the proportion of carbon, which I assign to sugar, must at all events be nearly correct.

We have no direct method of determining the weight of an integrant particle of sugar; but if the accuracy of the preceding analysis be admitted, it furnishes us with an indirect one, which cannot be rejected; for it is clear, that the atoms of oxygen, carbon, and hydrogen, will be to each other respectively, as the numbers $\frac{64}{8}$, $\frac{28}{3.5}$, $\frac{8}{1}$; and these numbers reduced to their lowest terms become 5, 3, 4, nearly, which being primes with respect to each other, must represent the number of atoms, of which an integrant particle of sugar is composed. Sugar then is a compound of 12 atoms; namely, five of oxygen, three of carbon, and four of hydrogen; the weight of an integrant particle of it is 47.5, and its symbol is $5w + 3c + 4h$. It differs from oxalic acid merely in containing an additional atom of oxygen and two of hydrogen. If we had any method of removing these substances, without altering the proportion of the other constituents, we should

obtain a much greater quantity of oxalic acid from sugar than we can at present; but nitric acid acts by removing one-half of the carbon in the form of carbonic acid; the sugar deprived of this, resolves itself into oxalic acid and water. Suppose two particles of sugar acted on at once, the symbol for them will be $10\,w + 6\,c + 8\,h$. Let three atoms of the carbon be removed by the action of the nitric acid, there will remain $10\,w + 3\,c + 8\,h$. Now

A particle of oxalic acid	$= 4\,w + 3\,c + 2\,h$
Six particles of water	$= 6\,w + \quad + 6\,h$
	$10\,w + 3\,c + 8\,h$

which is just the quantity of oxalic acid left. This will give us some idea of the way in which the formation of oxalic acid by nitric acid is accomplished. And although the series of changes is probably more complicated, yet they are ultimately equivalent to the preceding statement. I allude to the formation of malic acid, which is said to precede the oxalic acid, and afterwards to be converted into it by the subsequent action of nitric acid; but on the composition and formation of this latter acid, I avoid making any observations at present, as I propose to make them the subject of a separate dissertation.

V. *On Super-acid and Sub-acid Salts.* By William Hyde Wollaston, *M. D. Sec. R. S.*

Read January 28, 1808.

In the paper which has just been read to the Society, Dr. Thomson has remarked, that oxalic acid unites to strontian as well as to potash in two different proportions, and that the quantity of acid combined with each of these bases in their super-oxalates, is just double of that which is saturated by the same quantity of base in their neutral compounds.

As I had observed the same law to prevail in various other instances of super-acid and sub-acid salts, I thought it not unlikely that this law might obtain generally in such compounds, and it was my design to have pursued the subject with the hope of discovering the cause to which so regular a relation might be ascribed.

But since the publication of Mr. Dalton's theory of chemical combination, as explained and illustrated by Dr. Thomson,* the inquiry which I had designed appears to be superfluous, as all the facts that I had observed are but particular instances of the more general observation of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom singly, or, if either is in excess, it exceeds by a ratio to be expressed by some simple multiple of the number of its atoms.

* Thomson's Chemistry, 3d Edition, Vol. III. p. 425.

However, since those who are desirous of ascertaining the justness of this observation by experiment, may be deterred by the difficulties that we meet with in attempting to determine with precision the constitution of gaseous bodies, for the explanation of which Mr. DALTON's theory was first conceived, and since some persons may imagine that the results of former experiments on such bodies do not accord sufficiently to authorize the adoption of a new hypothesis, it may be worth while to describe a few experiments, each of which may be performed with the utmost facility, and each of which affords the most direct proof of the proportional redundance or deficiency of acid in the several salts employed.

Sub-carbonate of Potash.

Exp. 1. Sub-carbonate of potash recently prepared, is one instance of an alkali having one-half the quantity of acid necessary for its saturation, as may thus be satisfactorily proved.

Let two grains of fully saturated and well crystallized carbonate of potash be wrapped in a piece of thin paper, and passed up into an inverted tube filled with mercury, and let the gas be extricated from it by a sufficient quantity of muriatic acid, so that the space it occupies may be marked upon the tube.

Next, let four grains of the same carbonate be exposed for a short time to a red heat; and it will be found to have parted with exactly half its gas; for the gas extricated from it in the same apparatus will be found to occupy exactly the same space, as the quantity before obtained from two grains of fully saturated carbonate.

Sub-carbonate of Soda.

Exp. 2. A similar experiment may be made with a saturated carbonate of soda, and with the same result; for this also becomes a true semi-carbonate by being exposed for a short time to a red heat.

Super-sulphate of Potash.

By an experiment equally simple, super-sulphate of potash may be shewn to contain exactly twice as much acid as is necessary for the mere saturation of the alkali present.

Exp. 3. Let twenty grains of carbonate of potash (which would be more than neutralized by ten grains of sulphuric acid) be mixed with about twenty-five grains of that acid in a covered crucible of platina, or in a glass tube three quarters of an inch diameter, and five or six inches long.

By heating this mixture till it ceases to boil, and begins to appear slightly red hot, a part of the redundant acid will be expelled, and there will remain a determinate quantity forming super-sulphate of potash, which when dissolved in water will be very nearly neutralized by an addition of twenty grains more of the same carbonate of potash; but it is generally found very slightly acid, in consequence of the small quantity of sulphuric acid which remains in the vessel in a gaseous state at a red heat.

In the preceding experiments, the acids are made to assume a determinate proportion to their base, by heat which cannot destroy them. In those which follow, the proportion which a destructible acid shall assume cannot be regulated by the same means; but the constitution of its compounds previously formed, may nevertheless be proved with equal facility.

Super-oxalate of Potash.

Exp. 4. The common super-oxalate of potash is a salt that contains alkali sufficient to saturate exactly half of the acid present. Hence, if two equal quantities of salt of sorrel be taken, and if one of them be exposed to a red heat, the alkali which remains will be found exactly to saturate the redundant acid of the other portion.

In addition to the preceding compounds, selected as distinct examples of binacid salts, I have observed one remarkable instance of a more extended and general prevalence of the law under consideration; for when the circumstances are such as to admit the union of a further quantity of oxalic acid with potash, I found a proportion, though different, yet analogous to the former, regularly to occur.

§. *Quadroxalate of Potash.*

In attempting to decompose the preceding super-oxalate by means of acids, it appeared that nitric or muriatic acids, are capable of taking only half the alkali, and that the salt which crystallizes after solution in either of these acids, has accordingly exactly four times as much acid as would saturate the alkali that remains.

Exp. 5. For the purpose of proving that the constitution of this compound has been rightly ascertained, the salt thus formed should be purified by a second crystallization in distilled water; after which the alkali of thirty grains must be obtained by exposure to a red heat, in order to neutralize the redundant acid contained in ten grains of the same salt. The quantity of unburned salt contains alkali for one part out of

four of the acid present, and it requires the alkali of three equal quantities of the same salt to saturate the three remaining parts of acid.

The limit to the decomposition of super-oxalate of potash by the above acids, is analogous to that which occurs when sulphate of potash is decomposed by nitric acid; for in this case also, no quantity of that acid can take more than half the potash, and the remaining salt is converted into a definite super-sulphate, similar to that obtained by heat in the third experiment.

It is not improbable that many other changes in chemistry, supposed to be influenced by a general redundance of some one ingredient, may in fact be limited by a new order of affinities taking place at some definite proportion to be expressed by a simple multiple. And though the strong power of crystallizing in oxalic acid, renders the modifications of which its combinations are susceptible more distinct than those of other acids, it seems probable that a similar play of affinities will arise in solution, when other acids exceed their base in the same proportion.

In order to determine whether oxalic acid is capable of uniting to potash in a proportion intermediate between the double and quadruple quantity of acid, I neutralized forty-eight grains of carbonate of potash with thirty grains of oxalic acid, and added sixty grains more of acid, so that I had two parts of potash of twenty-four grains each, and six *equivalent* quantities of oxalic acid of fifteen grains each, in solution, ready to crystallize together, if disposed to unite, in the proportion of three to one; but the first portion of salt that crystallized, was the common binoxalate, or salt of sorrel, and a

portion selected from the after crystals (which differed very discernibly in their form) was found to contain the quadruple proportion of acid. Hence it is to be presumed, that if these salts could have been perfectly separated, it would have been found, that the two quantities of potash were equally divided, and combined in one instance with two, and in the other with the remaining four out of the six *equivalent* quantities of acid taken.

To account for this want of disposition to unite in the proportion of three to one by Mr. DALTON's theory, I apprehend he might consider the neutral salt as consisting of

2 particles potash with 1 acid,

The binoxalate as 1 and 1, or 2 with 2,

The quadroxalate as 1 and 2, or 2 with 4,

in which cases the ratios which I have observed of the acids to each other in these salts would respectively obtain.

But an explanation, which admits the supposition of a double share of potash in the neutral salt, is not altogether satisfactory; and I am further inclined to think, that when our views are sufficiently extended, to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.

For instance, if we suppose the limit to the approach of particles to be the same in all directions, and hence their virtual extent to be spherical (which is the most simple hypothesis); in this case, when different sorts combine singly there is but one mode of union. If they unite in the proportion of

two to one, the two particles will naturally arrange themselves at opposite poles of that to which they unite. If there be three, they might be arranged with regularity, at the angles of an equilateral triangle in a great circle surrounding the single spherule; but in this arrangement, for want of similar matter at the poles of this circle, the equilibrium would be unstable, and would be liable to be deranged by the slightest force of adjacent combinations; but when the number of one set of particles exceeds in the proportion of four to one, then, on the contrary, a stable equilibrium may again take place, if the four particles are situated at the angles of the four equilateral triangles composing a regular tetrahedron.

But as this geometrical arrangement of the primary elements of matter is altogether conjectural, and must rely for its confirmation or rejection upon future inquiry, I am desirous that it should not be confounded with the results of the facts and observations related above, which are sufficiently distinct and satisfactory with respect to the existence of the law of simple multiples. It is perhaps too much to hope, that the geometrical arrangement of primary particles will ever be perfectly known; since even admitting that a very small number of these atoms combining together would have a tendency to arrange themselves in the manner I have imagined; yet, until it is ascertained how small a proportion the primary particles themselves bear to the interval between them, it may be supposed that surrounding combinations, although themselves analogous, might disturb that arrangement, and in that case, the effect of such interference must also be taken into the account, before any theory of chemical combination can be rendered complete.

VI. *On the Inconvertibility of Bark into Alburnum.* By Thomas Andrew Knight, *Esq. F. R. S. In a Letter to Sir* Joseph Banks, *K. B. P. R. S.*

Read February 4, 1808.

MY DEAR SIR,

In a letter which I had the honour to address to you in the end of the last year,* I endeavoured to prove that the matter which composes the bark of trees, previously exists in the cells both of their bark and alburnum, in a fluid state, and that this fluid, even when extravasated, is capable of changing into a pulpous and cellular, and ultimately a vascular substance: the direction taken by the vessels being apparently dependent on the course which the descending fluid sap is made to take.† The object of the present Memoir is to prove, that the bark thus formed, always remains in the state of bark, and that no part of it is ever transmuted into alburnum, as many very eminent naturalists have believed.

Having procured, by grafting, several trees of a variety of

* Phil. Trans. 1807.

† I had observed this circumstance in many successive seasons; but I was not by any means prepared to believe that such an arrangement could take place in the coagulum afforded by an extravasated fluid; and I am indebted to Mr. Carlisle for having pointed out to me many circumstances in the motion and powers of the blood of animals, which induced me to give credit to the accuracy of my observations; and to that Gentleman and to Mr. Home, I have also subsequently to acknowledge many obligations.

the apple and crab tree, the woods of which were distinguishable from each other by their colours, I took off, early in the spring, portions of bark of equal length, from branches of equal size, and I transposed these pieces of bark, inclosing a part of the stem of the apple tree with a covering of the bark of the crab tree, which extended quite round it, and applying the bark of the apple tree to the stem of the crab tree in the same manner. Bandages were then applied to keep the transposed bark and the alburnum in contact with each other; and the air was excluded by a plaister composed of bees-wax and turpentine, and with a covering of tempered clay.

The interior surface of the bark of the crab tree presented numerous sinuosities, which corresponded with similar inequalities on the surface of the alburnum, occasioned by the former existence of many lateral branches. The interior surface of the bark of the apple tree, as well as the external surface of the alburnum, was, on the contrary, perfectly smooth and even. A vital union soon took place between the transposed pieces of bark, and the alburnum and bark of the trees to which they were applied; and in the autumn it appeared evident, that a layer of alburnum had been, in every instance, formed beneath the transposed pieces of bark, which were then taken off.

Examining the organization of the alburnum, which had been generated beneath the transposed pieces of bark of the crab tree, and which had formed a perfect union with the alburnum of the apple tree, I could not discover any traces of the sinuosities I had noticed; nor was the uneven surface of the alburnum of the crab tree more changed by the smooth transposed bark of the apple tree. The newly generated

alburnum, beneath the transposed bark, appeared perfectly similar to that of other parts of the stock, and the direction of the fibres and vessels did not in any degree correspond with those of the transposed bark.*

Repeating this experiment, I scraped off the external surface of the alburnum in several spaces, about three lines in diameter, and in these spaces no union took place between the transposed bark and the alburnum of the stock, nor was there any alburnum deposited in the abraded spaces; but the newly generated cortical and alburnous layers took a circular, and rather elliptical, course round those spaces, and appeared to have been generated by a descending fluid, which had divided into two currents when it came into contact with the spaces from which the surface had been scraped off, and to have united again immediately beneath them.

In each of these experiments, a new cortical and alburnous layer was evidently generated; and apparently by the same means that similar substances were generated beneath a plaister composed of bees-wax and turpentine, in former experiments;† and the only obvious difference in the result appears to be, that the transposed and newly generated bark formed a vital union with each other: and it is sufficiently evident, that if bark of any kind was converted into alburnum, it must have been that newly generated. For it can scarcely be sup-

* Duhamel having taken off, and immediately replaced, similar pieces of the bark of young elms, subsequently found that the alburnum, which was generated beneath such pieces of bark, had not formed any union with the alburnum of the tree, beneath it. But this great naturalist did not employ ligatures of sufficient power to bring the bark and alburnum into close contact, or the result would have been different.

† Phil. Trans. for 1807.

posed, that the bark of a crab tree was transmuted into the alburnum of an apple tree, or that the sinuosities of the bark of the crab tree could have been obliterated, had such transmutation taken place. There is not, however, any thing in the preceding cases, calculated to prove that the newly generated bark was not converted into alburnum; and the elaborate experiments of DUHAMEL sufficiently evince the difficulty of producing any decisive evidence in this case; nevertheless I trust that I shall be able to adduce such facts as, in the aggregate, will be found nearly conclusive.

Examining almost every day, during the spring and summer, the progressive formation of alburnum in the young shoots of an oak coppice, which had been felled two years preceding, I was wholly unable to discover any thing like the transmutation of bark into alburnum. The commencement of the alburnous layers in the oak (*quercus robur*) is distinguished by a circular row of very large tubes. These tubes are of course generated in the spring; and during their formation, I found the substance through which they passed to be soft and apparently gelatinous, and much less tenacious and consistent than the substance of the bark itself; and, therefore, if the matter which gave existence to the alburnum previously composed the bark, it must have been, during its change of character, nearly in a state of solution; but it is the transmutation of one organized substance into the other, and not the identity only of the matter of both, for which the disciples of MALPIGHI contend; and if the fibres and vessels of the bark really became those of the alburnum, a very great degree of similarity ought to be found in the organization of those substances. No such similarity, however, exists; and not any

thing at all corresponding with the circular row of large tubes in the alburnum of the oak is discoverable in the bark of that tree. These tubes are also generated within the interior surface of the bark, which is well defined; and during their formation the vessels of the bark are distinctly visible, as different organs; and had the one been transmuted into the other, their progressive changes could not, I think, possibly have escaped my observation: nor does the organization of the bark in other instances, in any degree indicate the character of the wood that is generated beneath it: the bark of the wych elm *(ulmus montana)* is extremely tough and fibrous; and it is often taken from branches of six or eight years old, to be used instead of cords; that of the ash *(fraxinus excelsior)* on the contrary, when taken from branches of the same age, breaks almost as readily in any one direction as in another, and scarcely presents a fibrous texture; yet the alburnum of these trees is not very dissimilar, and the one is often substituted for the other in the construction of agricultural instruments.

Mirbel has endeavoured to account for the dissimilar organization of the bark, and of the wood into which he conceives it to be converted, by supposing that the cellular substance of the bark is always springing from the alburnum, whilst the tree is growing, and that it carries with it part of the tubular substance *(tissu tubulaire)* of the liber, or interior bark. These parts of the interior bark, which are thus removed from contact with the alburnum, he conceives to constitute the external bark or cortex, whilst the interior part of the liber progressively changes into alburnum.

But if this theory (which I believe I have accurately stated,

though I am not quite certain that I fully comprehend its author*) were well founded, the texture of the alburnum must surely be much more intricate and interwoven than it is, and its tubes would lie less accurately parallel with each other than they do: and were the fibrous substance of the bark progressively changing into alburnum, the bark must of necessity be firmly attached to the alburnum during the spring and summer by the continuity, and indeed identity of the vessels and fibres of both these substances. This, however, is not in any degree the case, and the bark is in those seasons very easily separated from the alburnum; to which it appears to be attached by a substance that is apparently rather gelatinous than fibrous or vascular: and the obvious fact, that the adhesion of the cortical vessels and fibres to each other is much more strong than the adhesion of the bark to the alburnum, affords another circumstance almost as inconsistent with the theory of MALPIGHI, as with that of MIRBEL.

Many of the experiments of DUHAMEL are, however, apparently favourable to the theory of MALPIGHI, respecting the conversion of bark into alburnum; and MIRBEL has cited two, which he appears to think conclusive.† In the first of these, DUHAMEL shews that pieces of silver wire, inserted in the bark of trees, were subsequently found in their alburnum; but DUHAMEL himself has shewn, with his usual acuteness and candour, that the evidence afforded by this experiment is extremely defective; and he declares himself to be uncertain that the pieces of wire did not, at their first insertion, pass between the bark and the alburnum; in which case they would

* Chap. III. Article 5, *Traité d'Anatomie et de Physiologie Végétale.*

† Chap. III. Article 5.

necessarily have been covered by every successive layer of alburnum, without any transmutation of bark into that substance.*

In the second experiment cited by MIRBEL, DUHAMEL has shewn that when a bud of the peach tree, with a piece of bark attached to it, is inserted in a plum stock, a layer of wood perfectly similar to that of the peach tree will be found, in the succeeding winter, beneath the inserted bark. The statement of DUHAMEL is perfectly correct; but the experiment does not by any means prove the conversion of bark into wood; for if it be difficult to conceive (as he remarks) that an inserted piece of bark can deposit a layer of alburnum, it is at least as difficult to conceive how the same piece of bark can be converted into a layer of alburnum of more than twice its own thickness (and the thickness of the alburnum deposited frequently exceeds that of the bark in this proportion), without any perceptible diminution of its own proper substance. The probable operation of the inserted bud, which is a well organized plant, at the period when it becomes capable of being transposed with success, appears also, in this case, to have been overlooked, for I found that when I destroyed the buds in the succeeding winter, and left the bark which belonged to them uninjured, this bark no longer possessed any power to generate alburnum. It nevertheless continued to live, though perfectly inactive, till it became covered by the successive alburnous layers of the stock; and it was found many years afterwards inclosed in the wood. It was, however, still bark, though dry and lifeless, and did not appear to have made any progress towards conversion into wood.

* *Physique des Arbres*, Lib. IV. Ch. III.

In the course of very numerous experiments, which were made to ascertain the manner in which vessels are formed in the reproduced bark, * many circumstances came under my observation which I could adduce in support of my opinion, that bark is never transmuted into alburnum; but I do not think it necessary to trouble you with an account of them; for though much deference is certainly due to the opinions of those naturalists who have adopted the opposite theory, and to the doubts of DUHAMEL, I am not acquainted with a single experiment which warrants the conclusions they have drawn; and I think that were bark really transmuted into alburnum, its progressive changes could only have escaped the eyes of prejudiced, or inattentive observers. In the course of the ensuing spring, I hope to address to you some observations respecting the manner in which the alburnum is generated.

I am, my dear Sir,

your most obliged obedient servant,

Elton, Dec. 29, 1807. THOMAS AND. KNIGHT.

* Phil. Trans. for 1807.

VII. *Some Account of Cretinism.* *By* Henry Reeve, *M. D. of Norwich.* *Communicated by* William Hyde Wollaston, *M. D. Sec. R. S.*

Read February 11, 1808.

FELIX PLATER, in one of his observations, gives the history of a species of mental imbecility, which he saw in passing through the village of Bremis in the Valais. Cretinism, a word of uncertain derivation, is the name employed by the inhabitants of Switzerland to denote this disease, which is endemial in several districts of that country. It had probably existed long in those parts; for PLATER mentions cretins as being very common both in the Valais and in Carinthia, but the peculiar marks of these wretched beings were not generally known before he described them.* Mons. DE SAUSSURE has furnished the most minute and accurate account both of the appearances of the disorder, and of the circumstances which seem to produce it; and Mr. COXE and several travellers have noticed the symptoms of cretinism, without adducing any satisfactory explanation of the causes to which it may be ascribed. MALACARNI of Turin and Professor ACKERMANN have given a very accurate description of several cretins that they dissected; and besides some detached essays by different authors, a very full account of this malady is to be found in

* *F.* PLATERI *Praxeos Medicæ, Cap.* III. *Basil.* 1656.

an "*Essai sur le Goitre et Cretinisme par M.* FODERE," published at Paris in 1800.

My curiosity led me some time ago to inquire more particularly into the nature and causes of cretinism, because it is usually connected with goitre, or bronchocele; I was indeed led to this inquiry, partly by the hope of discovering some function for the thyroid gland, more satisfactory than what is commonly alledged; but in these expectations I have been disappointed.

In the summer of 1805, I had an opportunity of seeing several cretins at Martigny and Sion, and other villages in the Valais; and I was glad to compare what had been written upon that subject, with what my own observation could suggest. By inquiries on the spot, I intended to learn what connection subsisted between weakness of the intellectual faculties and the swelling of the thyroid gland: what were the moral and physical circumstances which could influence the condition of the inhabitants, so as to make idiocy so prevalent; and what were the most efficient modes of relief. The following results I beg leave to lay before the Royal Society.

Cretinism is found not only in the vallies of the Alps, both on the French and Italian side of these mountains, but in the mountainous parts of Germany and Spain; and it was observed in Chinese Tartary by Sir GEORGE STAUNTON, in a part of that country much resembling Switzerland and Savoy in its alpine appearance. The enlargement of the thyroid gland called goitre, is the most striking feature in the unsightly aspect of a cretin; but this is not a constant attendant. His head also is deformed, his stature diminutive, his complexion sickly, his countenance vacant and destitute of meaning, his

lips and eye-lids coarse and prominent, his skin wrinkled and pendulous, his muscles loose and flabby. The qualities of his mind correspond to the deranged state of the body which it inhabits; and cretinism prevails in all the intermediate degrees, from excessive stupidity to complete fatuity.

At a small village, not far distant from Martigny, I examined four cretins. One lad, twelve years old, could speak a few words; he was of a weak and feeble frame, silly, but had no goitre. Another boy, nine years old, was deaf and dumb, idiotic, with no goitre, the only child of his mother, who has a large goitre which affects her respiration and her voice, though in other respects she is intelligent and well formed, and the father enjoys good health; they are not natives of this place. I saw a family in which all the children were cretins; the eldest died a year ago, a miserable object; the second, a girl, twelve years old, is deaf and dumb and cross-eyed, and has a monstrous goitre, with just intelligence enough to comprehend a few natural signs; the third, is a boy six years old, small and feeble, abdomen enlarged, no goitre, very feeble in mind and body, not entirely deficient in understanding; the mother had a moderate sized goitre, but was quite free from any mental affection; the father neither goitrous nor stupid, but of a delicate constitution.

There is no necessary connexion between goitre and cretinism, notwithstanding the assertions and ingenious reasoning adduced by FODERE. It is probable, the one has been assumed as the cause of the other, from the enlargement of the thyroid gland being a frequent occurrence in cretins; and as it forcibly strikes the observer from the deformity it occasions, this strong impression may have converted an accidental,

though frequent occurrence, into a general and necessary cause. Cretinism is frequently observed without any affection of the thyroid gland, and that gland is often very much enlarged without any affection of the intellectual faculties. There seems to be some similarity between cretinism and rickets, as they both take place in infancy, are both characterized by feebleness of body, and sooner or later by feebleness of mind, and they both affect males and females equally; but there is no sort of connexion between persons afflicted with bronchocele in England, and with rickets. For although it might be granted, that there is some delicacy of frame in females about the period of pubescence when bronchocele usually occurs, yet neither irregular formation of the bones, nor weakness of the intellectual powers, are common symptoms attending bronchocele in Britain.

To what peculiarities then, in the physical constitution of certain districts, are we to ascribe the production of this singular malady? SAUSSURE's description of the Valais is exceedingly precise and accurate, and the causes which he has alledged appear sufficient to account for the phenomena. The vallies where cretinism is most frequent, are surrounded by very high mountains; they are sheltered from the currents of air, and exposed to the direct and still more to the reflected rays of the sun. The effluvia from the marshes are very strong, and the atmosphere humid, close, and oppressive. All the cretins that I saw, were in adjoining houses, in the little village called La Batia, situated in a narrow corner of the valley, the houses being built up under ledges of the rocks, and all of them very filthy, very close, very hot, and miserable habitations. In villages situated higher up the mountains,

no cretins are to be seen, and the mother of one of the children told me, of her own accord, without my asking the question, that her child was quite a different being when he was up the mountain, as she called it, for a few days.

The production of cretinism, by the bad quality of the air and the food, the neglect of moral education, and other evils attendant upon poverty, is supported by facts so pointed, that the greater number of cases in mountainous districts where snow-water abounds, may safely be ascribed to these general causes. The notion of snow-water being the cause of goitre, and consequently of cretinism, seems to have been derived from PLINY (Lib. II. cap. 37,) and copied by almost every succeeding writer, because it coincided with their hypotheses of cold and crude matters, although directly contradicted by facts. In the first place, persons born in places contiguous to the glaciers, who drink no other water than what flows from the melting of ice and snow, are not subject to this disorder; and, secondly, the disorder is observed in places where snow is unknown.

The theory of water impregnated with calcareous matter being the cause, is equally unfounded; because the common waters of Switzerland excel those of every other country in Europe for purity and flavour. There is not a village, nor a valley, but what is enlivened by limpid rivulets or streams gushing from the rocks. The water usually drank at La Batia and Martigny is from the river Dranse, which flows from the glacier of St. Bernard, and falls into the Rhone; it is remarkably free from earthy matter, and well tasted. At Martigny, there are two or three pumps, the water of which is pure and equally fit for culinary purposes, but said to be

unwholesome, without any good reason. At Bern, the water is extremely pure, yet, as HALLER remarks, swellings of the throat are not uncommon in both sexes, although cretinism is rare. With regard to the alledged causes of goitre, the general opinion of its being endemial in mountainous countries, is of no value, because the disease is rare in Scotland, and very common in the county of Norfolk.

The causes of cretinism begin to operate upon the system soon after, perhaps even before birth; the want of energy in the parent is communicated to the offspring; the children become deformed and cachectic very early in life, the growth and developement of the body is impeded, the abdomen becomes enlarged, and the glands swelled in various degrees; and the powers of the mind remain dormant, or become entirely obliterated, partly from want of proper organization, and partly from the total neglect of every thing like education.

It might be expected, that the dissection of cretins would throw some light upon the series of phenomena associated together in the origin and progress of this singular affection; but the people are so superstitious, that it is very difficult to procure bodies for anatomical examination. However, some dissections have been made, and the appearances in the cranium are very curious. From the description of a cretin's skull by ACKERMANN, it appears that the cavity for the reception of the pons varolii and medulla oblongata was completely obliterated, and that in which the cerebellum is lodged so much diminished, that it scarcely exceeded one-third of its natural capacity. The return of the venous blood must have been considerably impeded by the mal-confirmation of the foramina.

Appearances nearly similar were observed by MALACARNI and by FODERE.

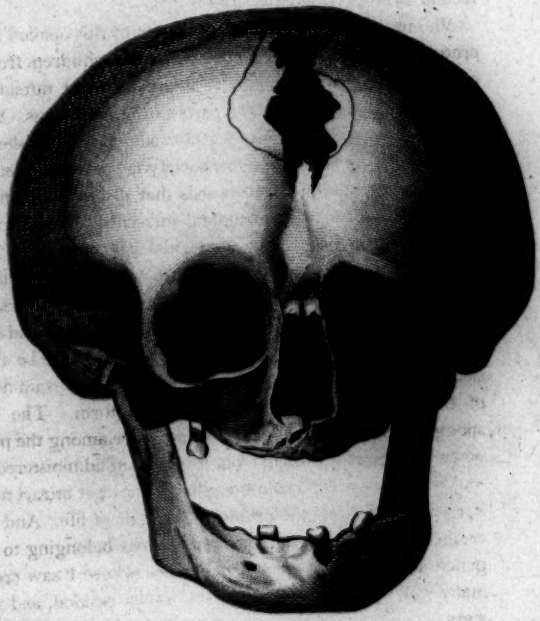
In the anatomical museum at Vienna, I saw a cretin's skull, from which Professor PROCHASKA was so obliging as to permit me to have two drawings taken. It is the cranium of a cretin, who died at the age of thirty, yet the fontanelle is not closed, the second set of teeth are not out of their sockets, and none of the bones are distinctly and completely formed. The head is very large, the face small; it is like the skull of an adult joined to the face of a child; every part bears marks of irregularity in the growth and formation; and irregular action must have been the concomitant of such a morbid structure, whether the appearances be considered as cause or effect.

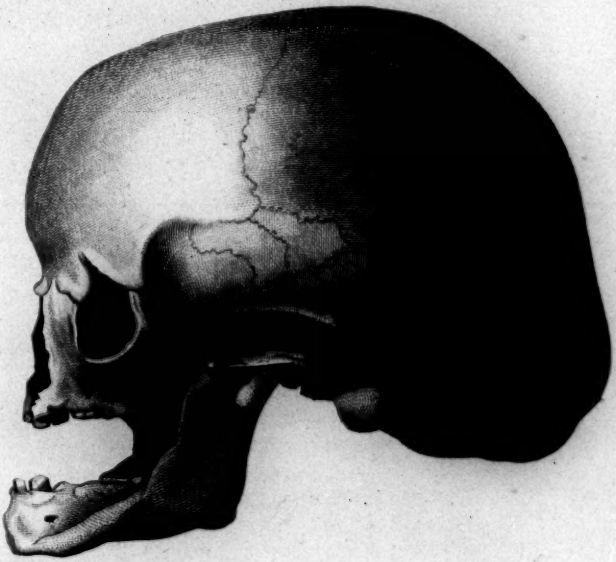
The four angles of the os malæ are not well defined; the zygomatic and maxillary processes of this bone are wanting; the nasal processes of the superior maxillary bone are very large, and exhibit no marks of union with the os malæ; the ossa nasi are very small; the temporal bone is imperfectly formed; the zygomatic process terminates at the coronoid process of the lower jaw; the mastoid and styloid processes are wanting, and the pars petrosa remarkably small; the squamous portion not distinctly marked; the os occipitis unusually large, and numerous additional bones, ossa triquetra, along the whole course of the lambdoid suture. These appearances will be readily seen by referring to the figures; the other deviations of the natural structure corresponded with those already described by different writers.

There is no fact in the natural history of man, that affords an argument so direct and so impressive, in proof of the in-

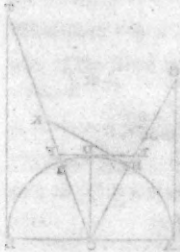
fluence of physical causes on the mind, as cretinism. It shows moreover, that the growth of every part is essentially connected with the conditions in which it is fit to exercise its peculiar functions; and in this respect, it fares with the intellectual as with the bodily powers.

The most decisive argument in proof of this opinion is, that cretinism may be prevented by removing children from the confined and dirty places where it prevails, and nursing and educating them in the higher parts of the mountains. Within these last ten years, the number of cretins has diminished, the condition of the lowest class of society is somewhat bettered, and more attention is paid towards that diseased constitution which is the forerunner of mental imbecility. I did not find that the poor creatures took any pride in having any of their children idiots or *bien heureux*, as some authors assert; on the contrary, the parents were very much ashamed of acknowledging that any cretins belonged to their families; and it was after repeated attempts, only by declaring myself to be a physician, that I could get access into their houses to examine any of these wretched beings in the human form. The burnt sponge is known as a remedy for the goitre among the people where it is most prevalent; but it is seldom administered, because the disease is so common, that it does not attract notice, nor affect in general the ordinary functions of life. And as to cretinism, that seems to be looked upon as belonging to indigence and poverty; for in every place where I saw cretins, many well looking persons of both sexes resided, and these were, without exception, persons of a higher class in society, who lived in better houses, and could supply both their moral and physical necessities.





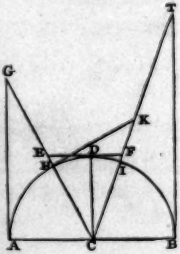
I might perhaps have insisted more upon the analogy between cretinism and rickets, for there is a remarkable coincidence in the literary history of these two diseases, as well as in many other points. GLISSON first described rickets, as it appeared in this country, in the middle of the seventeenth century, about the same time that PLATER mentions cretinism. The origin of both names is equally obscure; and since some of the remote causes are now discovered, it is to be hoped the diseases themselves will gradually disappear, and in some happier age be known only by description.



VIII. *On a new Property of the Tangents of the three Angles of a Plane Triangle.* *By Mr.* William Garrard, *Quarter Master of Instruction at the Royal Naval Asylum at Greenwich. Communicated by the Astronomer Royal.*

Read February 11, 1808.

PROPOSITION I. In every acute angled plane triangle, the sum of the three tangents of the three angles multiplied by the square of the radius, is equal to the continued product of the tangents.



Demonstration.—Let AH, HI, and IB be the arches to represent the given angles; and AG, HK, and BT be their tangents, put r the radius, AG $= a$, and BT $= b$,

Then $\frac{r^2}{a}$ and $\frac{r^2}{b}$ will be the tangents of HD and DI.

Now by Prop. VIII. Sect. I. Book I. EMERSON'S Trigonometry,

As radius square—product of two tangents
Is to radius square,
So is the sum of the tangents
To the tangent of their sum.

$$\therefore r^2 - \frac{r^4}{ab} : r^2 :: \frac{r^2}{a} + \frac{r^2}{b} : \frac{r^2a + r^2b}{ab - r^2} = \text{HK};$$

therefore $a + b + \frac{r^2a + r^2b}{ab - r^2} = \frac{a^2b + ab^2}{ab - r^2}$ = the sum of the three tangents,

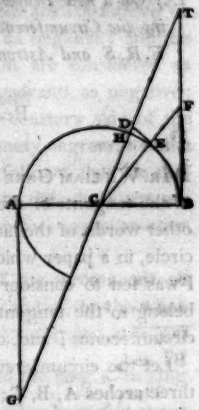
and $\frac{a^2b + ab^2}{ab - r^2} \times r^2 = ab \times \frac{r^2a + r^2b}{ab - r^2}$ = their continued product.

Q. E. D.

PROPOSITION II. In every obtuse angled plane triangle, the sum of the three tangents of the three angles multiplied by the square of radius, is equal to their continued product.

Demonstration.—Let AH be an obtuse arc, and HE, ED the other two.

Then BF, ED, and AG are the three tangents.



Put BF $= t$ and DE $= u$ radius $= r$, then per trigonometry, as before,

$r^2 \times \frac{t+u}{r^2-tu} = \mathrm{BT}$;

But $-\mathrm{BT} = \mathrm{AG} = -\frac{t+u}{r^2-tu} \times r^2$.

Wherefore $t + u - \frac{t+u}{r^2-tu} \times r^2 =$ the sum of the three tangents, which being reduced

is $= -tu \times \frac{t+u}{r^2-tu}$, and multiplied into r^2 is equal to

$tu \times -\frac{t+u}{r^2-tu} \times r^2 =$ the product.

Q. E. D.

IX. *On a new Property of the Tangents of three Arches trisecting the Circumference of a Circle, by* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read February 18, 1808.

Mr. William Garrard having shewn me a curious property of the tangents of the three angles of a plane triangle, or in other words, of the tangents of three arches trisecting a semicircle, in a paper which I have communicated to this Society, I was led to consider whether a similar property might not belong to the tangents of three arches trisecting the whole circumference; and, on examination, found it be so.

Let the circumference of a circle be divided any how into three arches A, B, C; that is, let A + B + C be equal to the whole circumference. I say, the square of the radius multiplied into the sum of the tangents of the three arches A, B, C, is equal to the product of the tangents multiplied together. I shall demonstrate this by symbolical calculation, now commonly called (especially by foreign mathematicians) analytic calculation.

It may be proper to premise, that the signification of the symbolical expressions of the tangents of an arc, whether with respect to geometry or numbers, are to be understood according to their position as lying on one side, or the other side of the radius, passing through the point of commencement of the arc of the circle; those tangents which belong to

the first or third quadrant of the circle being considered as positive, and those belonging to the second and fourth quadrant, being of a contrary direction, as negative; in like manner as the sines in the first semi-circle are considered as positive, and in the second semi-circle as negative; and the cosines in the first and fourth quadrant are considered as positive, and in the second and third quadrants as negative; they lying, in the second case, on the contrary side of the diameter passing through the point of ninety degrees, to what they do in the former. Hence it easily follows, that the tangent of any arch and of its supplement to the whole circumference, or 360 degrees, are equal and contrary to one another, or the one negative of the other.

Let t, u, w, be put for the tangents of the three arches A, B, C respectively, and r for the radius, and $\odot$ for the whole circumference. Then $A + B + C = \odot$, and $C = \odot - \overline{A+B}$. By trigonometry, $t, \overline{A+B} = \frac{r^2 \times t+u}{r^2-tu}$, and the tang. C = tang. $(\odot - \overline{A+B}) = -$ tang. $\overline{A+B}$, by what has been said above. Therefore $t, A+t$, $B+t$, C or $t+u+w = t+u-\frac{r^2 \times \overline{t+u}}{r^2-tu}$ $= tu \times -\frac{r^2 \times t+u}{r^2-tu}$; but t and u are the expressions for the tangents of A and B respectively, and $-\frac{r^2 \times \overline{t+u}}{r^2-tu}$ is the expression for the tangent of C, or for w. Therefore, $r^2 \times \overline{t+u+w}$, or the square of the radius multiplied into the sum of the three tangents of A, B, and C $= tuw$, or the product of the tangents. Q. E. D.

X. *An Account of the Application of the Gas from Coal to economical Purposes. By Mr.* William Murdoch. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 25, 1808.

THE facts and results intended to be communicated in this Paper, are founded upon observations made, during the present winter, at the cotton manufactory of Messrs. PHILIPS and LEE at Manchester, where the light obtained by the combustion of the gas from coal is used upon a very large scale; the apparatus for its production and application having been prepared by me at the works of Messrs. BOULTON, WATT, and Co. at Soho.

The whole of the rooms of this cotton mill, which is, I believe, the most extensive in the United Kingdom, as well as its counting-houses and store-rooms, and the adjacent dwelling-house of Mr. LEE, are lighted with the gas from coal. The total quantity of light used during the hours of burning, has been ascertained, by a comparison of shadows, to be about equal to the light which 2500 mould candles of six in the pound would give; each of the candles, with which the comparison was made consuming at the rate of 4-10ths of an ounce (175 grains) of tallow per hour.

The quantity of light is necessarily liable to some variation, from the difficulty of adjusting all the flames, so as to be perfectly equal at all times; but the admirable precision and

exactness with which the business of this mill is conducted, afforded as excellent an opportunity of making the comparative trials I had in view, as is perhaps likely to be ever obtained in general practice. And the experiments being made upon so large a scale, and for a considerable period of time, may, I think, be assumed as a sufficiently accurate standard for determining the advantages to be expected from the use of the gas lights under favourable circumstances.

It is not my intention, in the present Paper, to enter into a particular description of the apparatus employed for producing the gas; but I may observe generally, that the coal is distilled in large iron retorts, which during the winter season are kept constantly at work, except during the intervals of charging; and that the gas, as it rises from them, is conveyed by iron pipes into large reservoirs, or gazometers, where it is washed and purified, previous to its being conveyed through other pipes, called mains, to the mill. These mains branch off into a variety of ramifications (forming a total length of several miles), and diminish in size, as the quantity of gas required to be passed through them becomes less. The burners, where the gas is consumed, are connected with the above mains, by short tubes, each of which is furnished with a cock to regulate the admission of the gas to each burner, and to shut it totally off when requisite. This latter operation may likewise be instantaneously performed, throughout the whole of the burners in each room, by turning a cock, with which each main is provided, near its entrance into the room.

The burners are of two kinds: the one is upon the principle of the ARGAND lamp, and resembles it in appearance; the other is a small curved tube with a conical end, having

three circular apertures or perforations, of about a thirtieth of an inch in diameter, one at the point of the cone, and two lateral ones, through which the gas issues, forming three divergent jets of flame, somewhat like a fleur-de-lis. The shape and general appearance of this tube, has procured it among the workmen, the name of the cockspur burner.

The number of burners employed in all the buildings, amounts to 271 ARGANDS, and 633 cockspurs; each of the former giving a light equal to that of four candles of the description abovementioned; and each of the latter, a light equal to two and a quarter of the same candles; making therefore the total of the gas light a little more than equal to that of 2500 candles. When thus regulated, the whole of the above burners require an *hourly* supply of 1250 cubic feet of the gas produced from cannel coal; the superior quality and quantity of the gas produced from that material having given it a decided preference in this situation, over every other coal, notwithstanding its higher price.

The time during which the gas light is used, may, upon an average of the whole year, be stated at least at two hours per day of twenty-four hours. In some mills, where there is over work, it will be three hours; and in the few where night-work is still continued, nearly twelve hours. But taking two hours per day as the common average throughout the year, the consumption in Messrs. PHILIPS' and LEE's mill, will be $1250 \times 2 = 2500$ cubic feet of gas per day; to produce which, seven hundred weight of cannel coal is required in the retort. The price of the best Wigan cannel (the sort used) is $1\frac{1}{2}d.$ per cwt. (22*s.* 6*d.* per ton), delivered at the mill, or say about eight shillings for the seven hundred weight. Multiplying by

the number of working days in the year (918), the annual consumption of cannel will be 110 tons, and its cost £125.

About one-third of the above quantity, or say forty tons of good common coal, value ten shillings per ton, is required for fuel to heat the retorts; the annual amount of which is £20.

The 110 tons of cannel coal when distilled, produce about 70 tons of good coak, which is sold upon the spot at 1*s.* 4*d.* per cwt. and will therefore amount annually to the sum of £93.

The quantity of tar produced from each ton of cannel coal is from eleven to twelve ale gallons, making a total annual produce of about 1250 ale gallons, which not having been yet sold, I cannot determine its value; but whenever it comes to be manufactured in large quantities, it cannot be such as materially to influence the economical statement, unless indeed new applications of it should be discovered.

The quantity of aqueous fluid which came over in the course of the observations which I am now giving an account of, was not exactly ascertained, from some springs having got into the reservoir; and as it has not been yet applied to any useful purpose, I may omit further notice of it in this statement.

The interest of the capital expended in the necessary apparatus and buildings, together with what is considered as an ample allowance for wear and tear, is stated by Mr. Lee at about £550. per annum: in which some allowance is made for this apparatus being made upon a scale adequate to the supply of a still greater quantity of light, than he has occasion to make use of.

He is of opinion, that the cost of attendance upon candles would be as much, if not more, than upon the gas apparatus; so that in forming the comparison, nothing need be stated upon that score, on either side.

The economical statement for one year then stands thus:

Cost of 110 tons of cannel coal	-	£125.
Ditto of 40 tons of common ditto	-	20
		145
Deduct the value of 70 tons of coak	-	93
The annual expenditure in coal, after deducting the value of the coak, and without allowing any thing for the tar, is therefore	-	52
And the interest of capital, and wear and tear of apparatus	-	550

making the total expence of the gas apparatus, about £600. per annum.

That of candles, to give the same light, would be about £2000. For each candle consuming at the rate of 4-10ths of an ounce of tallow per hour, the 2500 candles burning upon an average of the year two hours per day, would, at one shilling per pound, the present price, amount to nearly the sum of money abovementioned.

If the comparison were made upon an average of three hours per day, the advantage would be still more in favour of the gas light; the interest of the capital, and wear and tear of the apparatus continuing nearly the same as in the former case; thus,

$1250 \times 3 = 3750$ cubic feet of gas per day, which would be produced by $10\frac{1}{4}$ cwt. of cannel coals; this multiplied by the

number of working days, gives 168 tons per annum, which valued as before, amounts to - - £188.

And 60 tons common coal for burning under the retorts, will amount to - - 30

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Deduct 105 tons of coak at 26*s.* 8*d.* - 140

Leaving the expenditure in coal, after deduction of the coak, and without allowance for the tar, at - - - - 78

Adding to which the interest and wear and tear of apparatus, as before, the total annual cost will not be more than £650. whilst that of tallow, rated as before, will be £3000.

It will readily occur, that the greater number of hours the gas is burnt, the greater will be its comparative economy; although in extending it beyond three hours, an increase of some parts of the apparatus would be necessary.

If the economical comparison were made with oils, the advantages would be less than with tallow.

The introduction of this species of light into the establishment of Messrs. Philips and Lee, has been gradual; beginning in the year 1805, with two rooms of the mill, the counting-houses, and Mr. Lee's dwelling-house. After which, it was extended through the whole manufactory, as expeditiously as the apparatus could be prepared.

At first, some inconvenience was experienced from the smell of the unconsumed, or imperfectly purified gas, which may in a great measure be attributed to the introduction of successive improvements in the construction of the apparatus, as the work proceeded. But since its completion, and since the persons to

whose care it is confided, have become familiar with its management, this inconvenience has been obviated, not only in the mill, but also in Mr. LEE's house, which is most brilliantly illuminated with it, to the exclusion of every other species of artificial light.

The peculiar softness and clearness of this light, with its almost unvarying intensity, have brought it into great favour with the work people. And its being free from the inconvenience and danger, resulting from the sparks and frequent snuffing of candles, is a circumstance of material importance, as tending to diminish the hazard of fire, to which cotton mills are known to be much exposed.

The above particulars, it is conceived, contain such information, as may tend to illustrate the general advantages attending the use of the gas light; but nevertheless the Royal Society may perhaps not deem it uninteresting to be apprized of the circumstances which originally gave rise in my mind to its application, as an economical substitute for oils and tallow.

It is now nearly sixteen years, since, in a course of experiments I was making at Redruth in Cornwall, upon the quantities and qualities of the gases produced by distillation from different mineral and vegetable substances, I was induced by some observations I had previously made upon the burning of coal, to try the combustible property of the gases produced from it, as well as from peat, wood, and other inflammable substances. And being struck with the great quantities of gas which they afforded, as well as with the brilliancy of the light, and the facility of its production, I instituted several experiments with a view of ascertaining the cost at which it might

be obtained, compared with that of equal quantities of light yielded by oils and tallow.

My apparatus consisted of an iron retort, with tinned copper and iron tubes through which the gas was conducted to a considerable distance; and there, as well as at intermediate points, was burned through apertures of varied forms and dimensions. The experiments were made upon coal of different qualities, which I procured from distant parts of the kingdom, for the purpose of ascertaining which would give the most economical results. The gas was also washed with water, and other means were employed to purify it.

In the year 1798, I removed from Cornwall to Messrs. Boulton, Watt, and Co's. works for the manufactory of steam engines at the Soho Foundry, and there I constructed an apparatus upon a larger scale, which during many successive nights was applied to the lighting of their principal building, and various new methods were practised, of washing and purifying the gas.

These experiments were continued with some interruptions, until the peace of 1802, when a public display of this light was made by me in the illumination of Mr. Boulton's manufactory at Soho, upon that occasion.

Since that period, I have, under the sanction of Messrs. Boulton, Watt, and Co. extended the apparatus at Soho Foundry, so as to give light to all the principal shops, where it is in regular use, to the exclusion of other artificial light; but I have preferred giving the results from Messrs. Philips' and Lee's apparatus, both on account of its greater extent, and the greater uniformity of the lights, which rendered the comparison with candles less difficult.

At the time I commenced my experiments, I was certainly unacquainted with the circumstance of the gas from coal having been observed by others to be capable of combustion; but I am since informed, that the current of gas escaping from Lord DUNDONALD's tar ovens had been frequently fired; and I find that Dr. CLAYTON, in a Paper in Volume XLI. of the Transactions of the Royal Society, so long ago as the year 1739, gave an account of some observations and experiments made by him, which clearly manifest his knowledge of the inflammable property of the gas, which he denominates "the spirit of coals;" but the idea of applying it as an economical substitute for oils and tallow, does not appear to have occurred to this gentleman, and I believe I may, without presuming too much, claim both the first idea of applying, and the first actual application of this gas to economical purposes.

XI. *Further Experiments on the Spleen.* By Everard Home, *Esq. F. R. S.*

Read February 25, 1808.

THE results of the experiments already brought forward having established the fact that fluids received into the stomach, when the pylorus is closed, pass through the spleen into the circulation of the blood; it became an object to determine, by experiment, whether this takes place when the parts are in a natural state.

The ass appeared, on many accounts, the best subject for this purpose, and as it is made use of to teach the veterinary pupils the anatomy of that tribe of animals, I applied to the Professor for permission to make my experiments in the theatre of the college.

This was granted me in the most obliging manner; the subjects were also supplied by the College, and Mr. SEWELL, the assistant Professor, gave me his personal aid with a degree of zeal and ability I have rarely met with, and have much pleasure in acknowledging.

In making the following experiments, I had the assistance of Mr. SEWELL, Mr. BRODIE, Mr. WILLIAM BRANDE, and Mr. CLIFT.

Experiment 1. An ass, which had been kept twenty-four hours without hay, to prevent the liquor that was to be poured into its stomach from being soaked up and retained there, on

the evening of the 3d of December, 1807, had a drench given it, consisting of half a pint of the spirituous tincture of rhubarb, diluted in half a pint of water. On the morning of the 4th, this was repeated at eight o'clock, and again at twelve. At two o'clock the animal was pithed, so as to destroy its sensibility, and before the circulation was entirely stopped, six ounces of blood were taken from the splenic vein into a graduated glass measure, and a similar quantity was taken from the left auricle of the heart, into a vessel of the same kind: these were allowed to coagulate and separate their serum.

The spleen was large and turgid; upon making sections of it, the cells were found to be very numerous; and towards the great end and near the edge, they were particularly distinct to the naked eye. The cut surface had a strong smell of rhubarb, and when it was applied to white paper wetted with the alkaline test, an orange tinge was produced. This was strongly contrasted by a stain made in the same manner with a section of the liver, which had no such tinge, nor did the liver give the slightest smell of rhubarb.

Infusions were made of the spleen and liver under similar circumstances; these were strained off into separate glasses, and tested by the alkali. The urine was tested in the same way. The serum, from the different portions of blood, was also poured off into separate glass vessels, to which the test was added. In nineteen hours after the blood had been taken from the veins, they were all compared together. The urine had so deep a tinge, that it nearly resembled the pure tincture of rhubarb in appearance; the others had a tinge, although in very different degrees; the quantity of rhubarb they contained

was estimated by adding tincture of rhubarb to alkaline water so as to produce corresponding tints. The infusion of spleen had a tint equal to sixty drops of tincture of rhubarb in two ounces of alkaline water: the serum of the splenic vein to fifteen drops: the serum from the left auricle of the heart, to three drops. The infusion of the liver gave no orange tinge, but had it not been obscured by the red particles of the blood, it must have been equal to that of the serum from the auricle.

The connecting membrane between the stomach and spleen was attentively examined, very few absorbent vessels were seen, and these were not in a turgid state, they were traced to the chain of glands situated near the edge of the spleen, which receive the absorbents of the stomach, but none were detected passing beyond the glands, nor did the glands admit quicksilver to pass through them towards the spleen.

Exp. 2. The former experiment was repeated upon another ass, with similar results, but less strongly marked; the cause of this difference was explained by the abdominal viscera being in an inflamed state.

The urine was less impregnated with rhubarb, the infusion of the spleen had a lighter tinge, and the serum of the splenic vein had it in a still less degree; but evidently exceeding that of the serum from the vena cava inferior opened just below the diaphragm, which was substituted for the left auricle of the heart, with a view to vary the experiment.

Exp. 3. The same experiment was made on a third ass with similar results.

Exp. 4. An ass that had been kept four days without water, and two without solid food, on the evening of the 8th of

January, 1808, had a ball given it, containing half an ounce of powdered rhubarb; on the 9th, at seven o'clock in the morning, this was repeated; a third was given at nine o'clock, and a fourth at twelve. At two o'clock the ass was pithed, and four ounces of blood were taken from the splenic vein, and the same quantity from the left auricle of the heart.

The spleen was found contracted to half the size of those in the former experiments; when cut into the cells were small, and it required a magnifying glass to see them distinctly. The substance was compact, and bore a near resemblance to a portion of liver; so that in this state the blood vessels, particularly the veins, must have been much contracted in their diameters.

The stomach contained about two ounces and a half of a gelatinous substance mixed with rhubarb, the small intestines were nearly empty, but the cæcum and colon contained several quarts of water, in which the rhubarb was more evident both to the sight and smell, than in the stomach.

The absorbent glands upon the edge of the colon were ranged in two rows, one on each side of the great vein, and were exceedingly numerous. In the space between these rows of glands, in some places twenty trunks of absorbent vessels could be readily counted, of a very large size.

The urine was impregnated with rhubarb, so as to acquire an orange tinge from the addition of the test; but the infusion of the spleen, and the serum of the different portions of blood, did not contain it in sufficient quantity to have the colour heightened by alkali.

Exp. 5. The last experiment was repeated upon another ass. Two ounces of blood were taken from the splenic vein,

two from the large vein of the colon, and two from the inferior vena cava in the lower part of the loins.

The spleen had the same appearance as in the last experiment.

The stomach contained nearly a pint of moderately solid contents, in which the rhubarb was very evident. The small intestines were nearly empty; but the cæcum and beginning of the colon contained several quarts of liquid, strongly impregnated with rhubarb.

The absorbent glands and vessels had the same appearance as in the former experiment.

The urine when tested was found impregnated with rhubarb.

The portions of serum of the blood taken from these different veins, when tested by the alkali, appeared to be very much alike; at least that from the splenic vein was not more tinged than the others.

Exp. 6. Having been informed by Mr. Sewell, that spirituous liquors given in large quantities to horses, produce inflammation of the brain, and sometimes death, and this information having been in some measure confirmed by an ass in a weakly state, that had taken half a pint of the spirituous tincture of rhubarb in the evening, dying in the night, I thought it right to make a comparative experiment with the infusion of rhubarb, to determine whether the result would be the same as with the tincture.

February 9, 1808. An ass had a pint of infusion of rhubarb given to it in the evening; the same dose was repeated at six o'clock in the morning of the 10th; and again at nine o'clock, and at twelve. At two o'clock the animal was pithed, and two

ounces of blood were taken from the splenic vein, two from the vein of the colon, and two from the inferior vena cava in the lower part of the loins.

The spleen was found turgid, and large; when the cut surface was rubbed on white paper, the orange tint was very evident without any test applied to it, particularly so, when compared with a similar stain made by a section of the liver, in which there was no such tinge.

In the stomach and duodenum, the rhubarb was found in large quantities; but none was met with in the cæcum.

The urine was impregnated with rhubarb, the orange tint upon the application of the alkali being very distinct.

At the end of twenty hours, the serum of the splenic vein had a tinge equal to four drops of the tincture of rhubarb in two ounces of alkaline water; that of the vein of the colon and vena cava was less distinct.

The effects of the infusion of rhubarb on the spleen, the serum of the blood and the urine corresponded exactly with that of the tincture in the former experiments, but was in a less degree of intensity.

In the course of these experiments, an attempt was made to ascertain whether the blood in the splenic vein has a greater proportion of serum than in the other veins of the body, and the general results were in favour of such an opinion; but it will appear, fom what follows, that the quantity of serum separated in twenty-four hours, is by no means a just criterion of the proportion, which the blood contains.

Experiment 1. Three ounces of blood from the arm of a healthy person were received into a graduated glass vessel, previously cooled to the temperature of 99°, three more into

a second glass of the temperature of 50°, and three into a third at 70°. The three glasses were brought into a room, the temperature of which varied from 40° to 50°. At the end of nineteen hours, the serum was found in the following quantities.

In the glass at 32° 9 drams.
50° 11
70° 10

The blood did not flow so freely into the glass at the highest temperature, as into the other two.

Exp. 2. This experiment was repeated, and the serum examined at the end of forty-three hours.

In the glass at 32° 12 drams.
50° 12
70° 13

Exp. 3. It was repeated, and the serum examined at the end of 67 hours.

In the glass at 32° 11 drams.
50° 11½
70° 11½

Exp. 4. It was repeated, and the serum measured at the end of ninety hours.

In the glass at 32° 11½ drams.
50° 13
70° 10½

The blood did not flow so readily into the glass at the highest temperature as into the other two.

From these experiments, it appears that the serum separates in larger quantity, when the blood is received into a vessel at the temperature of 70 degrees, than at 50° or 32°: this, how-

ever, is prevented from taking place by the blood not flowing readily from the vein.

From the experiments on the spleen contained in this, and the foregoing Paper, the following facts appear to have been ascertained.

That the spleen is met with in two very different states, one which may be termed the distended, the other the contracted, and that in the one its size is double what it is in the other. In the distended state there is a distinct appearance of cells containing a limpid fluid, distinguishable by the naked eye; in the contracted, these only become distinct when seen through a magnifying glass. The distended state takes place when the stomach has received unusual quantities of liquids before the animal's death; and the contracted state, when the animal has been kept several days without any drink before the spleen is examined.

That the trunk of the splenic vein (of the hog) is more than five times the size of the trunk of the splenic artery.

That when the pylorus is secured, coloured liquids pass from the cardiac portion of the stomach into the circulation of the blood, and go off by the urine; and while this is going on, the spleen is in its most distended state, and the colouring matter is found in its juices; although it is not to be detected in those of the liver. The colouring matter cannot therefore be conveyed to the spleen through the common absorbents of the stomach, which lead to the thoracic duct.

That when the pylorus is open, the colouring matter under the circumstances abovementioned is equally detected in the spleen.

That when the spleen is in this state, the blood in the splenic

vein has its serum more strongly impregnated with the colouring matter, than that of the blood in the other veins of the body; and when the stomach is kept without liquids, although colouring matter is carried into the system from the intestinal canal by the ordinary channels, no particular evidence of it is met with in the spleen or its veins.

That the cæcum and the portion of the colon immediately beyond it, is found (in the ass) to be at all times filled with liquids, even when none has been received into the stomach for several days, and there is a greater number of absorbent vessels for carrying liquids from the colon into the thoracic duct, than from any other part of the body. The colon is therefore a reservoir, from which the blood vessels are occasionally supplied with liquids.

Mr. SEWELL informs me, that the same observation applies in a still greater degree to the horse.

That coloured liquids taken into the human stomach, under some circumstances, begin to pass off by urine in seventeen minutes, continue to do so for some hours, and then disappear; they are again met with in the urine, after the colouring matter is known to have arrived at the great intestines, by its passing off by the bowels.

From the above facts, the following conclusions may be drawn.

That the liquids received into the stomach beyond what are employed for digestion, are not wholly carried out of it by the common absorbents of the stomach, or the canal of the intestines, but are partly conveyed through the medium of the spleen into the circulation of the liver.

The vessels which communicate between the stomach and

the spleen have not been discovered; but if it is proved that the colouring matter of the contents of the stomach, is met with in greater quantity in the spleen and in the vein which goes from that organ to the liver, than in the other veins of the body, there appears to be no other mode in which it can arrive there, but by means of such vessels; and the two different states of the spleen, which correspond with the quantities of liquids that pass from the stomach, are strongly in favour of the existence of such a channel.

This communication between the cardiac portion of the stomach, and the spleen, will explain the circumstance of those who are in the habit of drinking spirituous liquors having the spleen and liver so frequently diseased, and the diseases of both organs being of the same kind.

This organ is not essential to life, its office being of a secondary kind; but when it is materially diseased, or entirely removed, digestion must be disturbed. The extent to which this takes place, cannot be accurately known from experiments on quadrupeds, and the instances in which the human spleen has been removed have not been attended to with sufficient accuracy to afford an explanation of the effects that were produced on the stomach.

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METEOROLOGICAL JOURNAL

for January, 1807.

1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Jan. 1	34	8	0	34	52	30,53	63		NE	1	Fine.
	41	2	0	40	55	30,60	60		NE	1	Fine.
2	29	8	0	29	52	30,65	63		W	1	Foggy.
	32	2	0	32	53	30,56	63		SW	1	Foggy.
3	28	8	0	33	50	30,40	63		WNW	1	Cloudy.
	41	2	0	39	53	30,30	62		WSW	1	Fair.
4	36	8	0	40	50	30,27	65		W	1	Cloudy.
	42	2	0	41	53	30,35	60		NE	1	Fair.
5	37	8	0	37	49	30,50	60		NE	1	Cloudy.
	40	2	0	39	52	30,51	57		NE	1	Fair.
6	27	8	0	27	48	30,50	62		W	1	Cloudy.
	37	2	0	36	51	30,46	61		WSW	1	Fair.
7	31	8	0	33	47	30,38	62		W	1	Cloudy.
	40	2	0	40	50	30,35	60		WSW	1	Cloudy.
8	32	8	0	33	48	30,12	62		W	1	Fair.
	41	2	0	41	50	30,02	61		SSE	1	Fair.
9	37	8	0	42	50	29,84	65		ESE	1	Cloudy.
	47	2	0	47	53	29,78	65		ESE	1	Cloudy.
10	41	8	0	41	51	29,90	63		ENE	1	Cloudy.
	44	2	0	44	53	30,01	62		ENE	1	Fair.
11	35	8	0	35	50	30,24	65		NE	1	Cloudy.
	40	2	0	37	52	30,26	65		NE	1	Cloudy.
12	37	8	0	38	50	30,13	65		SW	1	Cloudy.
	46	2	0	44	52	30,04	64		SW	1	Cloudy.
13	43	8	0	44	50	29,72	60		W	1	Cloudy.
	45	2	0	44	52	29,77	58		NW	1	Cloudy.
14	29	8	0	30	49	30,04	50		NW	1	Fair.
	37	2	0	37	51	30,03	53		NW	1	Cloudy.
15	23	8	0	24	47	30,17	56		WNW	1	Cloudy.
	49	2	0	33	50	30,11	55		SW	1	Fair.
16	45	8	0	45	50	29,73	62	0,153	W	1	Fair.
	50	2	0	50	53	29,85	58		NW	1	Fair.

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for January, 1807.

1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
Jan. 17	46	8 0	46	52	29,96	67		W	1	Cloudy.
	51	2 0	51	54	29,85	67		WSW	1	Cloudy.
18	40	8 0	40	52	29,56	63		WNW	1	Cloudy.
	44	2 0	44	54	29,56	57		NW	1	Fair.
19	34	8 0	34	50	29,70	63		W	1	Fair.
	47	2 0	43	53	29,54	60		SSE	1	Cloudy.
20	36	8 0	36	51	29,04	64	0,200	SW	1	Fair.
	44	2 0	43	53	29,08	62		WSW	1	Cloudy.
21	35	8 0	37	51	29,04	63		SE	1	Cloudy.
	43	2 0	43	52	28,85	62		E	1	Cloudy.
22	35	8 0	37	51	29,33	65		SW	1	Cloudy.
	39	2 0	39	51	29,17	65		E	1	Rain.
23	36	8 0	36	51	29,58	64	0,140	SW	1	Fair.
	43	2 0	43	53	29,71	60		NW	1	Cloudy.
24	36	8 0	37	51	30,13	63		NW	1	Fair.
	45	2 0	45	53	30,22	60		N	1	Fair.
25	30	8 0	31	50	30,46	62		NW	1	Fair.
	40	2 0	40	51	30,47	62		WNW	1	Fair.
26	28	8 0	29	48	30,51	64		WNW	1	Fair.
	43	2 0	40	50	30,50	63		WSW	01	Cloudy.
27	40	8 0	42	48	30,50	60		N	01	Cloudy.
	45	2 0	45	53	30,54	58		NE	01	Fine.
28	36	8 0	36	50	30,64	64		NE	01	Cloudy.
	43	2 0	43	53	30,60	60		NNW	01	Cloudy.
29	34	8 0	34	50	30,43	63		WNW	1	Cloudy.
	42	2 0	42	53	30,36	64		NW	01	Cloudy.
30	32	8 0	38	50	30,14	63		W	01	Cloudy.
	41	2 0	41	53	30,03	60		SSW	01	Cloudy.
31	39	8 0	39	50	29,81	62		SW	01	Cloudy.
	44	2 0	44	53	29,65	58		SSW	01	Fine.

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for February, 1807.

1807	Sit'n Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Feb. 1	32	7	0	32	50	29,50	62		NE	2	Snow.
	38	2	0	38	51	29,70	62		NE	2	Cloudy.
2	25	7	0	25	48	29,70	61		SW	1	Fair.
	41	2	0	40	50	29,30	60		S	2	Cloudy.
3	32	7	0	32	48	28,98	65	0,395	WSW	1	Cloudy.
	40	2	0	40	53	29,07	60		W	1	Fair.
4	35	7	0	35	48	29,25	62		SW	1	Cloudy.
	40	2	0	40	51	29,24	63		S	1	Cloudy.
5	35	7	0	38	48	29,27	68		SSW	1	Rain.
	45	2	0	43	52	29,31	57		WNW	1	Cloudy.
6	35	7	0	36	49	29,44	64	0,085	SW	1	Fair.
	47	2	0	45	52	29,51	56		W	1	Cloudy.
7	35	7	0	36	50	29,65	60		W	1	Fair.
	44	2	0	44	52	29,88	55		NE	1	Fine.
8	36	7	0	39	50	29,83	63	0,060	S	2	Rain.
	46	2	0	46	52	29,56	66		S	2	Rain.
9	42	7	0	48	50	29,61	67	0,080	SW	1	Cloudy.
	52	2	0	52	54	29,48	67		WSW	2	Cloudy.
10	41	7	0	41	50	29,60	57	0,160	WNW	2	Fair.
	49	2	0	48	55	29,83	51		NW	2	Fair.
11	45	7	0	51	53	29,74	65	0,022	WSW	2	Cloudy.
	57	2	0	56	56	29,84	60		WSW	2	Fair.
12	52	7	0	52	55	30,05	60		WSW	2	Cloudy.
	57	2	0	57	57	30,17	55		WSW	1	Cloudy.
13	46	7	0	46	56	30,14	62		S	2	Cloudy.
	53	2	0	53	57	30,07	58		SSW	2	Hazy.
14	47	7	0	48	57	30,24	65		SSW	1	Cloudy.
	54	2	0	53	59	30,22	58		S	1	Fair.
15	44	7	0	44	57	30,11	60		S	1	Fine.
	55	2	0	54	58	30,00	55		S	1	Fine.
16	46	7	0	50	57	29,94	63		SSW	1	Cloudy.
	54	2	0	53	58	29,85	62		SSW	2	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
Feb. 17	43	7 0	42	57	29,80	60	0,022	WNW	1	Cloudy.
	47	2 0	47	57	29,66	53		WNW	2	Fair.
18	29	7 0	29	53	29,54	02	0,060	NE	2	Snow.
	31	2 0	31	54	29,67	55		NE	2	Fair.
19	26	7 0	26	51	30,06	58		NE	2	Fine.
	34	2 0	34	54	30,20	53		NE	2	Fair.
20	30	7 0	30	52	30,22	62		SW	1	Cloudy.
	47	2 0	47	54	30,05	56		SW	1	Cloudy.
21	38	7 0	38	52	29,80	62		WSW	1	Fair.
	47	2 0	47	55	29,80	53		WNW	1	Cloudy.
22	43	7 0	46	52	29,76	63		SW	2	Cloudy.
	50	2 0	50	55	29,64	58		SW	2	Cloudy.
23	39	7 0	39	52	29,74	55		WSW	1	Fair.
	45	2 0	45	56	29,85	49		WNW	1	Fine.
24	32	7 0	32	52	29,94	55		WSW	1	Fine.
	46	2 0	46	55	29,95	50		WSW	1	Hazy.
25	44	7 0	51	54	29,59	68	0,225	SW	2	Cloudy.
	57	2 0	57	56	29,56	60		WSW	1	Cloudy.
26	38	7 0	38	54	29,73	62		NE	1	Cloudy.
	42	2 0	41	55	29,88	51		NNW	1	Cloudy.
27	29	7 0	29	53	29,96	53		NW	1	Fine.
	38	2 0	38	54	30,02	54		NW	1	Cloudy.
28	30	7 0	33	52	30,41	62		NNE	2	Snow.
	39	2 0	39	54	30,50	60		NNE	2	Fair.

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1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°					°				
Mar. 1	31	7 0	31	51	30,60	64		NE	1	Fair.
	42	2 0	41	54	30,60	63		NE	1	Cloudy.
2	37	7 0	37	51	30,55	63		NE	1	Cloudy.
	44	2 0	44	53	30,50	57		NE	1	Cloudy.
3	38	7 0	38	51	30,23	65		NE	1	Cloudy.
	43	2 0	43	53	30,10	60		NE	1	Cloudy.
4	33	7 0	33	51	29,90	60		NE	1	Cloudy.
	44	2 0	43	53	29,85	57		NE	2	Cloudy.
5	25	7 0	26	49	29,85	57		NE	2	Cloudy.
	35	2 0	35	53	29,87	54		NE	2	Cloudy.
6	26	7 0	26	49	29,91	56		NW	2	Fine.
	38	2 0	37	52	29,80	48		NW	1	Cloudy.
7	29	7 0	30	49	29,86	63		NE	1	Snow.
	41	2 0	41	53	29,97	57		NE	2	Fair.
8	31	7 0	34	49	29,90	60		SW	1	Cloudy.
	47	2 0	46	52	29,64	57		SW	1	Cloudy.
9	36	7 0	37	49	29,35	64	0,195	NE	2	Rain.
	41	2 0	39	52	29,49	64		NE	2	Rain.
10	33	7 0	34	48	29,92	60		NE	2	Fair.
	41	2 0	38	52	30,23	52		NE	2	Snow.
11	31	7 0	33	48	30,14	60		NE	2	Cloudy.
	39	2 0	37	50	30,14	57		NE	2	Sleet.
12	35	7 0	35	47	30,26	55		NNE	2	Cloudy.
	45	2 0	43	51	30,26	52		NE	1	Cloudy.
13	37	7 0	38	48	30,19	56		NE	1	Cloudy.
	45	2 0	38	52	30,17	53		NNE	1	Cloudy.
14	35	7 0	37	48	30,12	61		NNE	2	Cloudy.
	45	2 0	41	51	30,07	56		NNE	2	Cloudy.
15	35	7 0	36	47	29,96	57		N	1	Cloudy.
	41	2 0	40	49	29,98	48		NNE	1	Fine.
16	27	7 0	30	44	29,94	55		NNW	2	Cloudy.
	40	2 0	39	48	29,50	51		W	2	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Mar. 17	29	7 0	30	44	29.45	57		NNW	2	Fine.
	40	2 0	37	47	29,52	52		NNE	2	Fine.
18	36	7 0	42	45	29.34	61		WNW	2	Fine.
	52	2 0	44	49	29,29	56		WNW	2	Cloudy.
19	35	7 0	37	45	29.55	57		WSW	2	Hazy.
	52	2 0	49	51	29,70	46		WSW	2	Cloudy.
20	34	7 0	36	49	30,04	53		W	1	Hazy.
	50	2 0	50	51	30,05	45		WNW	2	Fine.
21	39	7 0	42	49	30,18	63		NW	1	Cloudy.
	55	2 0	50	52	30,23	56		NW	1	Cloudy.
22	42	7 0	44	50	30,49	61		SSE	1	Cloudy.
	55	2 0	53	54	30,49	52		E	1	Fine.
23	37	7 0	40	50	30,49	57		NE	2	Fair.
	53	2 0	44	53	30,49	45		SE	1	Fine.
24	33	7 0	35	49	30,47	55		NE	2	Fair.
	44	2 0	44	52	30,46	48		NE	1	Fine.
25	32	7 0	36	49	30,34	54		NE	1	Cloudy.
	44	2 0	43	51	30,32	53		NE	1	Cloudy.
26	36	7 0	37	49	30,25	59		NE	1	Cloudy.
	45	2 0	42	51	30,15	56		NE	2	Cloudy.
27	39	7 0	40	50	29,87	61		NE	2	Hazy.
	44	2 0	41	51	29,85	59		NE	1	Cloudy.
28	38	7 0	39	49	29,76	60		NE	1	Cloudy.
	44	2 0	42	50	29,76	56		NE	1	Cloudy.
29	37	7 0	40	49	29,70	59		NE	2	Cloudy.
	47	2 0	44	50	29,75	51		NE	1	Hazy.
30	32	7 0	34	48	29,83	57		NE	1	Cloudy.
	45	2 0	43	49	29,82	45		NE	1	Snow.
31	35	7 0	36	47	29,69	57		NW	1	Cloudy.
	44	2 0	43	50	29,57	59		NW	1	

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for April, 1807.

1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°					°				
Apr. 1	34	7 0	36	48	29,51	63	0,230	NNW	1	Cloudy.
	45	2 0	45	50	29,58	58		NW	1	Cloudy.
2	32	7 0	34	47	29,75	64		NW	1	Snow.
	42	2 0	36	49	29,80	61		NW	1	Snow.
3	31	7 0	32	47	29,93	62		NE	2	Snow.
	43	2 0	37	49	29,97	51		NE	2	Fair.
4	31	7 0	32	47	30,12	61		NE	2	Cloudy.
	43	2 0	43	50	30,13	56		NNE	2	Fair.
5	32	7 0	40	48	30,20	55		NNW	1	Fine.
	51	2 0	44	50	30,18	47		NW	1	Fine.
6	33	7 0	40	48	30,16	59		WNW	2	Fine.
	53	2 0	51	52	30,09	45		NW	2	Fine.
7	35	7 0	37	50	30,08	54		NW	2	Cloudy.
	54	2 0	54	54	30,08	48		WNW	2	Fine.
8	43	7 0	46	52	30,23	61		NW	1	Cloudy.
	59	2 0	57	55	30,26	49		NW	1	Hazy.
9	44	7 0	46	54	30,31	59		NW	1	Cloudy.
	64	2 0	60	59	30,26	45		NW	1	Fine.
10	44	7 0	46	56	30,06	52		NW	1	Cloudy.
	63	2 0	55	58	29,92	52		NW	1	Cloudy.
11	49	7 0	49	57	29,70	60		NW	1	Cloudy.
	56	2 0	55	60	29,60	55		W	2	Cloudy.
12	49	7 0	50	57	29,40	54		SW	2	Cloudy.
	60	2 0	57	60	29,39	46		SW	2	Fine.
13	47	7 0	48	57	29,35	58		SW	2	Cloudy.
	60	2 0	57	61	29,30	52		S	2	Cloudy.
14	42	7 0	43	56	29,36	59		SW	1	Cloudy.
	56	2 0	55	60	29,36	50		SW	1	Cloudy.
15	42	7 0	42	56	29,36	62	0,190	NE	1	Rain.
	56	2 0	48	58	29,33	58		NE	2	Cloudy.
16	40	7 0	41	55	29,52	55		NE	2	Cloudy.
	47	2 0	45	58	29,62	50		NE	2	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	° o	o	Inches.		Inches.	Points.	Str.	
	°					°				
Apr. 17	33	7 0	34	54	29.78	55		NE	1	Snow.
	44	2 0	40	57	29.78	58		NE	1	Snow.
18	31	7 0	34	53	29.86	55		NE	2	Fair.
	45	2 0	38	57	29.87	50		NE	2	Fair.
19	31	7 0	36	52	29.98	55		NE	2	Fair.
	45	2 0	44	56	30.00	54		NE	2	Snow.
20	32	7 0	37	52	30,18	53		NE	2	Fair.
	44	2 0	43	55	30.20	50		NE	2	Cloudy.
21	32	7 0	36	51	30,21	50		NE	1	Cloudy.
	44	2 0	43	55	30.17	48		NE	1	Hazy.
22	34	7 0	38	53	29.92	50		NE	1	Hazy.
	53	2 0	51	53	29.92	49		SW	1	Hazy.
23	45	7 0	48	52	29.94	57	0,025	SW	1	Cloudy.
	61	2 0	60	58	29.95	48		SW	1	Fair.
24	45	7 0	51	56	30,08	57		SW	1	Cloudy.
	64	2 0	62	60	30,06	52		SW	1	Cloudy.
25	50	7 0	51	58	30,18	63		NNE	1	Cloudy.
	65	2 0	64	61	30,17	53		NE	1	Cloudy.
26	47	7 0	53	58	30,26	52		NNE	1	Fair.
	66	2 0	60	62	30,24	50		NE	1	Fair.
27	48	7 0	53	60	30,18	54		NE	1	Fair.
	77	2 0	77	66	30,10	42		SW	1	Fine.
28	55	7 0	56	63	30,15	57		W	1	Cloudy.
	71	2 0	70	65	30,15	49		W	1	Cloudy.
29	54	7 0	57	63	30,13	58		NW	1	Cloudy.
	71	2 0	68	66	30,07	50		NE	1	Hazy.
30	49	7 0	54	64	30,00	58		NE	1	Fair.
	71	2 0	71	67	29.93	50		NE	1	Fair.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points	Str.	Weather.
	°			°	°		°				
May 1	53	7	0	56	65	29,85	58		ENE	1	Fine.
	75	2	0	75	68	29,80	47		W	1	Fine.
2	57	7	0	60	65	29,80	54		SSW	1	Hazy.
	75	2	0	75	68	29,78	45		NW	1	Hazy.
3	53	7	0	56	65	29,78	56		SW	1	Hazy.
	76	2	0	70	67	29,68	43		WSW	1	Fine.
4	51	7	0	51	64	29,72	61		NE	2	Rain.
	59	2	0	56	66	29,68	57		NE	2	Cloudy.
5	53	7	0	53	64	29,45	62	0,290	E	1	Cloudy.
	68	2	0	67	66	29,34	50		E	1	Hazy.
6	53	7	0	54	64	29,10	57	0,135	SW	2	Cloudy.
	63	2	0	62	65	29,03	50		S	2	Cloudy.
7	51	7	0	52	62	29,18	54		SW	2	Cloudy.
	58	2	0	58	64	29,27	54		SW	2	Rain.
8	46	7	0	48	61	29,70	56	0,150	W	1	Rain.
	59	2	0	58	64	29,70	53		W	2	Cloudy.
9	52	7	0	55	61	29,38	65	0,242	S	2	Rain.
	59	2	0	59	62	29,35	63		S	2	Cloudy.
10	49	7	0	49	60	29,42	63	0,115	N	2	Rain.
	55	2	0	51	60	29,57	58		NW	2	Cloudy.
11	45	7	0	48	58	29,87	59	0,200	N	1	Rain.
	57	2	0	56	60	29,91	55		WSW	1	Rain.
12	50	7	0	52	58	29,67	65	0,570	SW	2	Rain.
	60	2	0	59	61	29,57	60		SW	2	Cloudy.
13	54	7	0	55	59	29,54	58		W	2	Cloudy.
	64	2	0	63	61	29,63	53		WNW	2	Cloudy.
14	48	7	0	50	59	29,81	63	0,265	NNE	1	Rain.
	58	2	0	57	62	29,72	65		WSW	1	Rain.
15	54	7	0	56	61	29,82	66	0,400	SW	1	Cloudy.
	68	2	0	67	63	29,85	54		SW	1	Cloudy.
16	49	7	0	56	61	30,06	55	0,058	WSW	1	Fair.
	67	2	0	64	62	30,09	51		W	1	Fair.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Baroм. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Str.	Weather.
May 17	53	7	0	56	61	30,14	56		SW	2	Cloudy.
	69	2	0	66	63	30,15	43		SW	1	Fair.
18	54	7	0	60	61	30,27	53		SSW	2	Fine.
	72	2	0	70	64	30,28	45		NNE	2	Fine.
19	45	7	0	50	61	30,41	52		NE	2	Fair.
	59	2	0	59	63	30,39	48		NE	2	Fair.
20	46	7	0	47	59	30,29	53		NE	2	Cloudy.
	63	2	0	56	61	30,21	51		ENE	2	Fair.
21	48	7	0	53	60	30,09	57		E	2	Fine.
	66	2	0	65	62	30,10	44		E	2	Fine.
22	45	7	0	50	60	30,22	55		E	2	Fine.
	66	2	0	65	63	30,22	50		E	2	Fine.
23	48	7	0	55	60	30,21	57		E	2	Fine.
	72	2	0	72	63	30,17	45		ENE	2	Fine.
24	55	7	0	64	62	30,05	55		ESE	2	Fine.
	81	2	0	79	65	29,99	42		SSE	2	Fine.
25	59	7	0	67	64	29,82	53		NE	1	Fair.
	84	2	0	83	67	29,77	41		NE	1	Fine.
26	60	7	0	60	66	29,79	53		WSW	2	Cloudy.
	71	2	0	71	67	29,78	50		SW	2	Fair.
27	56	7	0	58	65	29,86	53		NW	2	Cloudy.
	65	2	0	65	65	29,95	46		NW	2	Fair.
28	50	7	0	52	63	29,97	51		WNW	2	Fair.
	66	2	0	65	63	29,87	48		WNW	1	Fair.
29	47	7	0	50	62	29,74	53		NE	1	Fair.
	60	7	0	59	63	29,70	51		NE	2	Cloudy.
30	44	7	0	44	59	29,69	63	0,186	NNW	2	Rain.
	46	7	0	45	59	29,68	73		NNE	2	Rain.
31	45	7	0	48	58	29,86	55	0,264	ENE	2	Cloudy.
	54	7	0	54	59	29,90	53		ENE	2	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
June 1	° 48	7	0	50	58	29.89	72	0,320	ESE	1	Rain.
	58	2	0	57	61	29.84	53		SE	1	Cloudy.
2	48	7	0	51	59	29.93	62	0,253	WSW	1	Cloudy.
	62	2	0	60	61	29.97	54		W	1	Cloudy.
3	50	7	0	54	60	30.02	58		NE	1	Cloudy.
	61	2	0	61	62	30.04	51		NNE	1	Fair.
4	48	7	0	52	59	30.14	57		NE	2	Fair.
	64	2	0	61	61	30.17	50		NE	1	Cloudy.
5	50	7	0	53	60	30.14	61		NE	1	Cloudy.
	67	2	0	67	61	30.10	47		NE	1	Cloudy.
6	54	7	0	54	59	29.98	62		NE	1	Cloudy.
	58	2	0	55	61	29.93	61		SW	1	Cloudy. [lightning with thunder.
7	49	7	0	50	59	29.60	65	0,725	NE	1	Rain.
	58	2	0	58	59	29.70	55		NW	2	Cloudy.
8	50	7	0	52	59	29.64	61		NW	1	Cloudy.
	64	2	0	64	61	29.66	50		SW	2	Cloudy.
9	54	7	0	55	60	29.76	52		SW	2	Fair.
	66	2	0	65	61	29.80	43		SW	2	Fair.
10	49	7	0	53	59	30.04	56		WSW	2	Fair.
	65	2	0	65	61	30.10	49		SW	2	Cloudy.
11	58	7	0	59	60	30.05	63	0,054	SW	2	Cloudy.
	68	2	0	67	62	30.07	55		SW	2	Cloudy.
12	49	7	0	51	61	30.18	55		WSW	1	Fair.
	67	2	0	66	62	30.20	43		W	2	Fair.
13	51	7	0	54	60	30.24	55		W	2	Fine.
	70	2	0	69	62	30.26	46		SW	1	Fair.
14	51	7	0	55	59	30.16	58		W	1	Cloudy.
	73	2	0	73	62	30.10	49		WSW	1	Fair.
15	52	7	0	57	59	29.99	50		WSW	1	Fine.
	77	2	0	76	62	29.92	37		S	1	Fine.
16	56	7	0	60	62	29.74	49		W	1	Fair.
	76	2	0	74	65	29.72	44		SW	2	Fair.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
June 17	51	7	0	54	62	29.96	52		WNW	2	Fair.
	70	2	0	69	64	29.98	43		SW	2	Fair.
18	53	7	0	55	63	30.06	50		NNW	2	Fair.
	68	2	0	67	64	30.10	44		NW	2	Fair.
19	52	7	0	55	63	30.27	51		NNE	1	Fine.
	66	2	0	65	63	30.26	44		NW	4	Cloudy.
20	52	7	0	55	62	30.25	50		NW	1	Fine.
	70	2	0	66	64	30.22	43		W	1	Fair.
21	52	7	0	56	63	30.38	50		NNE	1	Fine.
	73	8	0	72	64	30.36	42		W	1	Fine.
22	55	7	0	59	64	30.22	49		NW	2	Fine.
	73	2	0	72	66	30.16	47		WNW	2	Fine.
23	57	7	0	61	65	30.11	50		NW	2	Fine.
	70	2	0	69	66	30.14	44		N	2	Fair.
24	57	7	0	60	65	30.03	55		NW	3	Cloudy.
	74	2	0	74	67	29.97	46		SSW	2	Fair.
25	56	7	0	60	65	29.87	52		NE	1	Fair.
	76	2	0	74	68	29.86	44		E	2	Fine.
26	55	7	0	58	65	29.95	53		NNE	2	Cloudy.
	68	2	0	68	68	29.97	50		E	1	Fair.
27	58	7	0	62	66	30.03	52		W	2	Fair.
	75	2	0	74	68	30.03	46		WNW	2	Fair.
28	56	7	0	60	66	30.05	48		NNE	2	Fine.
	72	2	0	72	68	30.05	46		NNE	1	Fair.
29	54	7	0	58	66	29.97	51		NNE	2	Fine.
	71	2	0	71	67	29.93	47		NNW	2	Fair.
30	51	7	0	55	65	29.92	51		NNE	2	Cloudy.
	67	2	0	67	66	29.89	47		NE	1	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°		°	°		°				
July 1	56	7 0	59	65	29,87	49		NNW	2	Cloudy.
	66	2 0	63	65	29,91	47		NW	1	Cloudy.
2	54	7 0	57	65	29,98	52		WNW	2	Fair.
	73	2 0	73	67	29,94	44		W	2	Fair.
3	57	7 0	58	65	29,93	51		W	2	Cloudy.
	67	2 0	67	66	29,90	48		W	2	Cloudy.
4	56	7 0	58	65	30,00	52		SW	1	Cloudy.
	68	2 0	67	66	30,01	48		NW	1	Cloudy.
5	53	7 0	57	64	30,06	55		N	2	Cloudy.
	71	2 0	71	66	30,04	45		N	1	Fair.
6	53	7 0	60	64	29,98	49		NNE	2	Fair.
	68	2 0	67	66	30,01	44		E	2	Cloudy.
7	48	7 0	55	63	30,18	49		NNE	2	Fair.
	64	2 0	63	65	30,22	46		NE	1	Cloudy.
8	48	7 0	54	64	30,28	50		NE	2	Fair.
	67	2 0	67	65	30,28	47		E	1	Fine.
9	51	7 0	57	64	30,27	51		NE	1	Fine.
	76	2 0	75	65	30,23	44		N	1	Fine.
10	58	7 0	62	64	30,12	51		SW	1	Fair.
	80	2 0	79	65	30,02	43		N	1	Fine.
11	60	7 0	64	66	29,84	53		E	2	Cloudy.
	78	2 0	76	70	29,79	50		E	2	Fair.
12	60	7 0	63	68	29,84	55		SSW	2	Cloudy.
	77	2 0	77	69	29,89	46		SW	2	Fair.
13	65	7 0	68	69	29,92	52		SSW	2	Fine.
	78	2 0	78	73	29,91	45		S	2	Fine.
14	62	7 0	64	69	29,99	52		SSW	2	Cloudy.
	71	2 0	67	69	29,98	53		SSW	2	Cloudy.
15	63	7 0	64	68	29,89	60	0,040	WSW	1	Cloudy.
	74	2 0	73	70	29,87	50		NNW	1	Fair.
16	58	7 0	61	68	29,94	56		W	2	Cloudy.
	75	2 0	74	70	29,95	50		W	2	Fair.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
July 17	55	7	0	58	68	30,03	58		WNW	1	Cloudy.
	75	2	0	75	70	30,00	50		NW	1	Fair.
18	62	7	0	64	69	30,08	54		WNW	1	Fair.
	76	2	0	75	70	30,10	44		WNW	1	Fair.
19	59	7	0	62	69	30,11	49		WNW	1	Fair.
	80	2	0	79	70	30,09	47		W	2	Fine.
20	64	7	0	66	69	29,95	54		WSW	2	Fair.
	80	2	0	79	72	29,90	48		SW	2	Fair.
21	59	7	0	62	70	29,94	52		W	1	Fair.
	79	2	0	76	71	29,91	46		S	1	Fair.
22	61	7	0	67	71	29,83	57		E	2	Fair.
	85	2	0	85	74	29,77	45		E	2	Fine.
23	67	7	0	68	72	29,66	52		S	2	Fair.
	78	2	0	76	73	29,72	47		S	2	Cloudy.
24	63	7	0	64	71	29,67	52		S	2	Fair.
	75	2	0	74	72	29,67	49		S	2	Cloudy.
25	58	7	0	64	69	29,87	53	0,090	S	2	Cloudy.
	73	2	0	72	70	29,89	48		SE	1	Cloudy.
26	62	7	0	65	70	29,77	54		S	1	Fine.
	75	2	0	70	70	29,78	50		S	1	Cloudy.
27	58	7	0	60	70	29,93	55		S	2	Fair.
	74	2	0	73	70	29,93	45		S	2	Fair.
28	57	7	0	58	69	30,03	54		SSW	2	Fair.
	78	2	0	78	70	30,00	44		S	1	Fine.
29	63	7	0	66	69	29,86	48		S	1	Cloudy.
	78	2	0	77	71	29,77	45		SSW	2	Cloudy.
30	65	7	0	66	70	29,61	63	0,205	SSW	2	Cloudy.
	73	2	0	72	70	29,68	46		SW	2	Cloudy.
31	62	7	0	63	68	29,71	55		SE	2	Cloudy.
	71	2	0	70	69	29,60	55		S	1	Rain.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points	Str.	Weather.
	°			°	°	Inches.		Inches.			
Aug. 1	58	7	0	59	67	29,76	58	0,055	S	2	Fine.
	72	2	0	72	68	29,81	47		S	2	Fair.
2	55	7	0	56	65	29,90	58		SW	1	Cloudy.
	75	2	0	74	67	29,86	45		SSW	1	Fair.
3	59	7	0	62	65	29,80	54		S	2	Cloudy.
	74	2	0	74	67	29,83	46		SW	1	Fair.
4	53	7	0	56	64	29,94	54		SW	2	Fine.
	72	2	0	72	66	29,95	44		SW	2	Fair.
5	55	7	0	58	64	29,99	53		SW	2	Fine.
	71	2	0	71	66	29,98	45		SW	1	Cloudy.
6	60	7	0	62	65	29,84	60		SSW	1	Cloudy.
	72	2	0	72	67	29,82	54		S	2	Cloudy.
7	62	7	0	62	67	29,84	64	0,016	S	1	Rain.
	71	2	0	70	67	29,88	57		S	1	Cloudy.
8	60	7	0	62	67	29,90	62	0,195	N	1	Cloudy.
	65	2	0	65	67	29,91	63		NNE	1	Rain.
9	58	7	0	58	66	30,02	63	0,083	NE	1	Cloudy.
	68	2	0	68	67	30,00	54		NE	1	Cloudy.
10	56	7	0	59	65	30,00	62		NE	1	Cloudy.
	68	2	0	67	67	30,00	53		NE	1	Cloudy.
11	54	7	0	56	65	29,94	60		E	1	Fair.
	75	2	0	75	68	29,81	50		ESE	1	Fair.
12	62	7	0	69	67	29,70	67	0,095	SW	1	Cloudy.
	74	2	0	73	68	29,73	52		S	2	Cloudy.
13	64	7	0	67	68	29,67	60		SE	1	Fair.
	72	2	0	72	71	29,70	50		S	2	Fair.
14	64	7	0	64	69	29,53	70	0,840	SSW	1	Rain.
	72	2	0	71	70	29,67	51		WSW	1	Cloudy.
15	59	7	0	61	68	29,91	60	0,200	SW	1	Fine.
	72	2	0	72	68	30,00	53		SW	1	Cloudy.
16	61	7	0	62	68	30,22	58		SW	1	Fine.
	75	2	0	73	69	30,22	53		SW	1	Cloudy.

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	°			°	°	Inches.	°	Inches.			
Aug. 17	64	7	0	65	68	30,24	60		N	1	Cloudy.
	78	2	0	78	70	30,19	50		E	1	Cloudy.
18	59	7	0	62	68	30,09	63		NE	1	Cloudy.
	77	2	0	77	71	30,01	50		ESE	1	Fair.
19	62	7	0	63	69	29,94	64		NE	1	Cloudy.
	81	2	0	78	71	29,91	50		SE	1	Cloudy.
20	61	7	0	62	70	29,91	63		S	1	Fair.
	76	2	0	74	71	29,93	51		SW	1	Cloudy.
21	64	7	0	67	70	29,87	56		E	1	Cloudy.
	80	2	0	78	71	29,84	52		S	1	Cloudy.
22	62	7	0	63	69	29,84	57		SW	1	Fine.
	80	2	0	80	72	29,81	50		SSW	1	Fair.
23	64	7	0	66	70	29,85	67		S	1	Fine.
	79	2	0	79	73	29,84	48		S	2	Fair.
24	65	7	0	66	70	29,85	60		SSW	1	Cloudy.
	72	2	0	70	70	29,81	58		SSW	1	Rain.
25	60	7	0	61	69	29,90	64	0,280	SW	1	Cloudy.
	76	2	0	76	70	29,93	51		SSW	1	Fair.
26	60	7	0	62	69	29,98	64		SW	1	Cloudy.
	75	2	0	75	71	30,01	52		SW	1	Fair.
27	59	7	0	62	69	30,01	57		SE	1	Fine.
	80	2	0	78	71	29,91	50		E	1	Fine.
28	64	7	0	66	71	29,70	60		SE	2	Cloudy.
	73	2	0	73	71	29,78	53		SSW	2	Cloudy.
29	59	7	0	60	69	29,92	61		S	1	Fine.
	72	2	0	68	70	29,91	54		S	2	Cloudy.
30	55	7	0	56	68	29,94	57		SSW	2	Fine.
	70	2	0	70	68	29,96	46		SSW	2	Fair.
31	52	7	0	53	65	30,06	56		SW	1	Fine.
	63	2	0	63	66	30,11	53		WNW	1	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 1	54	7	0	55	65	30,20	58		SW	1	Cloudy.
	68	2	0	67	66	30,21	50		NW	1	Cloudy.
2	51	7	0	53	62	30,21	58		WSW	1	Fine.
	70	2	0	69	65	30,18	48		WSW	1	Fine.
3	51	7	0	52	63	30,11	57		SW	1	Fine.
	70	2	0	70	66	30,03	47		SW	1	Fine.
4	53	7	0	57	62	29,90	58		SW	1	Cloudy.
	72	2	0	72	66	29,86	53		SW	1	Cloudy.
5	60	7	0	61	64	29,75	58		SSW	2	Cloudy.
	68	2	0	68	65	29,65	50		S	2	Cloudy.
6	57	7	0	57	63	29,38	55		S	2	Cloudy.
	63	2	0	62	62	29,46	48		SSW	2	Cloudy.
7	47	7	0	49	62	29,72	58		SW	1	Fine.
	63	2	0	63	63	29,77	46		W	1	Fair.
8	44	7	0	45	61	29,98	56		SW	1	Fine.
	62	2	0	60	62	30,01	47		W	1	Fair.
9	51	7	0	52	60	29,77	63	0,335	E	1	Rain.
	66	2	0	64	62	29,50	60		S	1	Rain.
10	52	7	0	53	60	29,88	61	0,365	WNW	1	Cloudy.
	58	2	0	57	62	30,00	54		NNW	1	Cloudy.
11	42	7	0	43	59	30,12	60		WSW	1	Fair.
	61	2	0	61	60	30,00	50		SW	1	Fair.
12	46	7	0	47	59	29,82	55		WSW	1	Cloudy.
	57	2	0	57	60	29,80	47		NW	1	Fair.
13	42	7	0	43	58	29,95	54		NW	1	Fine.
	55	2	0	53	59	29,98	46		NNE	2	Fair.
14	38	7	0	39	57	30,04	58		SW	1	Fine.
	57	2	0	57	58	29,96	48		W	1	Fair.
15	46	7	0	48	58	29,96	59		NNE	1	Fine.
	57	2	0	56	59	29,99	48		NE	1	Fair.
16	45	7	0	46	58	29,98	56		WSW	1	Cloudy.
	57	2	0	56	59	29,93	47		NW	1	Fair.

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	°			°	°	Inches.	°	Inches.			
Sep. 17	43	7	0	45	57	29,92	57		WNW	1	Fine.
	57	2	0	57	59	29,92	48		NE	1	Fair.
18	39	7	0	39	57	29,91	58		NE	1	Cloudy.
	58	2	0	58	60	29,83	48		NE	1	Fair.
19	45	7	0	46	58	29,90	60		NE	1	Fine.
	60	2	0	60	61	30,00	48		NE	1	Fine.
20	39	7	0	42	57	30,22	60		W	1	Fine.
	60	2	0	60	58	30,18	47		WNW	1	Cloudy.
21	52	7	0	52	58	30,05	62		WSW	1	Cloudy.
	61	2	0	60	60	29,96	56		SW	2	Cloudy.
22	56	7	0	56	59	29,75	66	0,105	SW	2	Cloudy.
	60	2	0	60	61	29,70	65		SW	1	Rain.
23	56	7	0	58	60	29,57	67	0,443	SW	2	Cloudy.
	61	2	0	60	62	29,54	68		SSW	2	Cloudy.
24	55	7	0	56	61	29,57	68	0,202	S	2	Cloudy.
	65	2	0	64	63	29,49	55		S	2	Cloudy.
25	51	7	0	52	61	29,41	63		SSW	1	Fair.
	63	2	0	60	62	29,35	60		S	2	Cloudy.
26	48	7	0	49	60	29,50	65	0,270	W	1	Fine.
	62	2	0	62	63	29,70	50		W	1	Fine.
27	51	7	0	58	60	29,48	70	0,122	S	2	Cloudy.
	66	2	0	65	63	29,44	53		WSW	2	Fair.
28	54	7	0	54	60	29,50	65	0,082	W	2	Fine.
	61	2	0	61	63	29,71	52		W	2	Fair.
29	45	7	0	47	60	29,95	60		WSW	2	Cloudy.
	61	7	0	56	61	29,74	60		SE	1	Rain.
30	56	7	0	57	61	29,50	63	0,016	WSW	2	Cloudy.
	60	7	0	60	62	29,64	50		WNW	2	Cloudy.

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	°			°	°	Inches.		Inches.			
Oct. 1	43	7	0	45	58	30,18	57		SW	1	Fine.
	56	2	0	56	60	30,17	50		S	1	Cloudy.
2	50	7	0	56	59	30,05	67	0,075	S	2	Cloudy.
	64	2	0	64	62	30,01	53		S	2	Fair.
3	53	7	0	55	60	30,06	72		SW	1	Foggy.
	64	2	0	64	63	30,13	56		SW	1	Fair.
4	53	7	0	54	60	30,15	70		S	1	Cloudy.
	65	2	0	65	63	30,14	58		S	1	Fine.
5	51	7	0	52	62	30,06	68		S	1	Foggy.
	64	2	0	64	63	30,03	64		SSW	1	Hazy.
6	57	7	0	57	62	29,98	67		W	1	Cloudy.
	60	2	0	60	63	30,01	60		SW	1	Cloudy.
7	54	7	0	57	62	30,00	66		SW	1	Cloudy.
	67	2	0	67	64	29,97	53		SSW	2	Fair.
8	58	7	0	58	63	29,79	60		SW	2	Cloudy.
	62	2	0	60	63	29,85	50		SW	2	Cloudy.
9	51	7	0	52	62	30,12	58		WSW	1	Cloudy.
	60	2	0	60	63	30,15	51		WNW	1	Cloudy.
10	54	7	0	57	62	30,10	72		SW	1	Cloudy.
	65	2	0	64	63	30,12	63		SW	1	Cloudy.
11	55	7	0	56	63	30,15	64		SW	1	Cloudy.
	62	2	0	62	62	30,13	60		SW	1	Cloudy.
12	57	7	0	57	62	30,02	64		S	1	Cloudy.
	63	2	0	63	62	30,00	58		SSW	1	Cloudy.
13	58	7	0	58	62	30,15	66		SW	1	Cloudy.
	66	2	0	66	63	30,20	57		SE	1	Cloudy.
14	58	7	0	59	63	30,20	67		S	1	Cloudy.
	68	2	0	67	62	30,18	58		S	1	Fair.
15	59	7	0	59	63	30,05	66		SW	1	Cloudy.
	65	2	0	65	65	30,05	60		SW	1	Cloudy.
16	51	7	0	52	62	30,15	65		WSW	1	Foggy.
	62	2	0	62	65	30,12	54		SW	1	Cloudy.

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		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Oct. 17	54	7	0	56	63	30,03	63		SW	1	Cloudy.
	64	2	0	63	63	29,97	57		SW	1	Cloudy.
18	58	7	0	60	62	29,90	70		SW	1	Cloudy.
	61	2	0	61	63	30,07	50		WNW	2	Fair.
19	44	7	0	45	61	30,30	60		SW	1	Fine.
	59	2	0	59	62	30,26	53		SSW	1	Fine.
20	50	7	0	53	61	29,93	64		S	1	Fair.
	63	2	0	63	63	29,78	54		S	1	Fair.
21	55	7	0	55	61	29,45	65		SE	2	Fair.
	62	2	0	62	63	29,46	58		S	2	Cloudy.
22	54	7	0	54	62	29,50	65	0,102	ESE	1	Rain.
	58	2	0	58	63	29,23	73		NE	1	Rain.
23	47	7	0	47	61	29,27	66	0,315	W	1	Cloudy.
	52	2	0	52	61	29,22	59		WNW	2	Cloudy.
24	40	7	0	41	59	29,35	63		NE	1	Cloudy.
	55	2	0	54	61	29,38	59		ESE	1	Cloudy.
25	45	7	0	47	59	29,58	65		E	1	Fine.
	55	2	0	55	60	29,66	58		ESE	1	Cloudy.
26	50	7	0	51	59	29,65	70	0,122	E	1	Rain.
	56	2	0	56	60	29,61	64		ENE	1	Cloudy.
27	42	7	0	44	58	29,46	68	0,033	N	1	Rain.
	53	2	0	52	60	29,50	66		N	1	Cloudy.
28	39	7	0	39	57	29,85	65	0,072	N	1	Fair.
	48	2	0	48	58	29,85	57		WNW	1	Fine.
29	41	7	0	43	57	29,64	67		WSW	1	Cloudy.
	53	2	0	53	58	29,55	63		W	1	Rain.
30	40	7	0	40	56	29,68	63	0,016	SW	1	Cloudy.
	53	2	0	50	57	29,43	66		SW	1	Cloudy.
31	45	7	0	45	56	29,68	60	0,032	N	1	Fair.
	50	2	0	50	58	29,88	64		N	1	Fair.

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1807	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Nov. 1	42	7	0	46	56	29.95	60		S	1	Cloudy.
	56	2	0	56	57	29.84	60		WSW	1	Fine.
2	47	7	0	47	57	29.66	68	0,125	SW	1	Cloudy.
	54	2	0	52	58	29.65	53		WSW	1	Fair.
3	42	7	0	42	57	29,24	56	0,090	SW	2	Fair.
	49	2	0	49	57	29,22	53		W	2	Fair.
4	40	7	0	42	56	29,35	62		SW	1	Cloudy.
	51	2	0	49	57	29,47	57		W	1	Fair.
5	36	7	0	38	54	29,61	63		WSW	1	Fair.
	53	2	0	53	56	29,43	61		SSW	1	Cloudy.
6	43	7	0	43	56	29,46	64		SW	1	Fair.
	50	2	0	50	58	29,54	63		WSW	1	Fine.
7	46	7	0	46	57	29,21	75	0,385	NW	1	Cloudy.
	53	2	0	52	58	29,35	60		W	1	Fair.
8	43	7	0	43	56	29,13	70	0,230	SW	1	Rain.
	47	2	0	47	57	29,12	63		S	1	Hazy.
9	37	7	0	37	54	29,37	63	0,070	SW	2	Fair.
	46	2	0	46	57	29,47	58		SW	1	Fair.
10	37	7	0	37	54	29,48	68	0,140	SW	1	Fair.
	49	2	0	48	57	29,20	55		SE	1	Cloudy.
11	36	7	0	37	53	29,04	66	0,505	SSW	1	Rain.
	43	2	0	43	53	29,40	62		NW	2	Fair.
12	32	7	0	32	52	29,83	60		N	1	Fine.
	39	2	0	39	53	29,91	63		NNE	1	Rain.
13	33	7	0	33	52	30,01	63		NE	1	Cloudy.
	39	2	0	39	53	30,00	61		N	1	Fair.
14	35	7	0	36	52	30,05	66	0,060	SW	1	Rain.
	43	2	0	43	52	30,08	65		NNE	1	Cloudy.
15	35	7	0	36	51	30,00	68		NE	1	Cloudy.
	46	2	0	46	54	29,92	61		NE	1	Cloudy.
16	41	7	0	44	51	29,90	62		NE	2	Cloudy.
	46	2	0	46	52	29,90	60		NE	1	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°						°				
Nov. 17	41	7	0	41	51	29.90	75	0,065	NE	1	Rain.
	44	2	0	44	54	29.85	63		NNE	1	Cloudy.
18	40	7	0	40	51	29.60	63		SW	1	Cloudy.
	42	2	0	41	53	29.60	60		N	1	Cloudy.
19	32	7	0	32	51	29.41	63		E	1	Snow.
	46	2	0	38	52	29.17	65		E	1	Snow.
20	39	7	0	43	50	28.68	65	0,280	S	3	Rain.
	43	2	0	41	52	28.80	62		SSW	2	Cloudy.
21	32	7	0	32	50	29.33	63		SW	1	Fair.
	40	2	0	38	53	29.44	60		SW	1	Fair.
22	30	7	0	31	49	29.60	65		NNE	1	Cloudy.
	46	2	0	40	53	29.33	67		E	1	Rain.
23	40	7	0	46	51	29.11	74	0,470	SSE	2	Cloudy.
	49	2	0	47	53	29.04	68		SE	2	Rain.
24	34	7	0	34	50	29.17	65	0,035	SSW	1	Fair.
	40	2	0	39	52	29.23	57		SW	1	Cloudy.
25	35	7	0	35	50	29.42	64		SW	2	Fine.
	42	2	0	42	53	29.47	63		S	1	Fine.
26	37	7	0	37	50	29.51	67		SSW	1	Cloudy.
	42	2	0	42	53	29.48	64		SW	1	Fair.
27	30	7	0	30	50	29.60	68		SW	1	Fair.
	36	2	0	36	54	29.72	56		W	1	Fine.
28	28	7	0	28	49	29.82	60		SW	1	Fair.
	33	2	0	33	52	29.77	58		N	1	Foggy.
29	31	7	0	33	50	29.62	63		NNE	1	Cloudy.
	36	2	0	36	51	29.59	63		NNE	1	Cloudy.
30	33	7	0	34	48	29.71	68		N	1	Cloudy.
	38	2	0	38	49	29.78	66		N	1	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.		Inches.			
Dec. 1	34	8	0	34	48	29.92	67		NE	1	Cloudy.
	39	2	0	38	51	29.91	67		E	1	Cloudy.
2	35	8	0	40	50	29.86	65		WSW	2	Cloudy.
	45	2	0	44	52	29.75	66		WSW	1	Cloudy.
3	33	8	0	33	49	29.90	58		WSW	1	Fair.
	38	2	0	38	52	29.97	55		NW	1	Fair.
4	34	8	0	43	50	29.86	65		SW	1	Fair.
	49	2	0	49	53	29.86	62		WSW	1	Fair.
5	40	8	0	43	50	29.70	66		SW	2	Cloudy.
	48	2	0	48	54	29.56	63		SW	2	Cloudy.
6	37	8	0	37	52	29.45	63		SW	1	Fine.
	43	2	0	43	54	29.49	58		WSW	1	Fine.
7	34	8	0	34	51	29.52	63		NE	1	Cloudy.
	37	2	0	36	53	29.59	61		NE	1	Cloudy.
8	23	8	0	23	48	29.58	55		NW	1	Cloudy.
	35	2	0	25	51	29.45	60		NW	1	Snow.
9	24	8	0	32	47	29.80	64		N	1	Cloudy.
	36	2	0	36	50	29.86	64		N	1	Cloudy.
10	26	8	0	26	47	30.00	62		N	1	Cloudy.
	32	2	0	32	50	30.00	60		WSW	1	Cloudy.
11	24	8	0	32	48	30.00	68		W	1	Cloudy.
	40	2	0	40	51	30.03	68		WNW	1	Cloudy.
12	37	8	0	37	48	30.20	70	0,225	W	1	Cloudy.
	43	2	0	42	52	30.22	68		W	1	Cloudy.
13	40	8	0	40	48	30.27	63		W	1	Cloudy.
	43	2	0	43	52	30.18	62		SW	1	Cloudy.
14	41	8	0	43	49	30.13	70		W	1	Cloudy.
	45	2	0	45	52	30.17	62		NW	1	Fair.
15	33	8	0	35	50	30.18	67		W	1	Foggy.
	39	2	0	38	53	30.13	67		W	1	Foggy.
16	38	8	0	38	48	30.04	63		W	1	Cloudy.
	40	2	0	40	53	30.07	62		SW	1	Cloudy.

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1807	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
	°			°	°						
Dec. 17	34	8	0	32	50	30,05	68		E	1	Foggy.
	36	2	0	36	52	30,03	67		E	1	Cloudy.
18	33	8	0	33	50	30,01	67		E	1	Cloudy.
	37	2	0	36	52	30,00	67		E	1	Cloudy.
19	36	8	0	37	51	30,08	67		E	1	Cloudy.
	37	2	0	37	52	30,12	68		E	1	Cloudy.
20	30	8	0	30	49	30,32	66		E	1	Cloudy.
	32	2	0	32	52	30,34	66		E	1	Cloudy.
21	29	8	0	29	48	30,41	70		E	1	Foggy.
	31	2	0	31	50	30,38	68		E	1	Cloudy.
22	28	8	0	31	46	30,26	70		E	1	Cloudy.
	37	2	0	37	50	30,24	64		NE	1	Cloudy.
23	32	8	0	32	47	30,16	70		W	1	Foggy.
	36	2	0	36	50	30,13	70		WSW	1	Cloudy.
24	35	8	0	38	47	30,14	70		SW	1	Cloudy.
	41	2	0	41	51	30,12	67		SSW	1	Cloudy.
25	38	8	0	40	49	30,12	67		SSW	1	Cloudy.
	43	2	0	42	52	30,03	65		S	1	Cloudy.
26	43	8	0	49	50	29,92	72		SW	1	Cloudy.
	51	2	0	50	53	29,88	74		SW	1	Cloudy.
27	47	8	0	47	52	29,70	75	0,125	SW	1	Cloudy.
	47	2	0	47	53	29,75	68		WSW	1	Cloudy.
28	36	8	0	41	51	29,56	67		SW	1	Cloudy.
	47	2	0	47	52	29,83	64		SSW	2	Cloudy.
29	47	8	0	48	52	29,54	67	0,025	S	2	Cloudy. [South wind last night.
	51	2	0	49	54	29,37	63		S	2	Cloudy.
30	42	8	0	46	52	29,53	67	0,085	SSW	2	Rain.
	46	2	0	46	52	29,61	61		SW	2	Cloudy.
31	36	8	0	42	51	29,65	68	0,016	S	2	Rain.
	49	2	0	49	53	29,56	67		S	2	Rain.

1807.	Sir J. Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	51	23	38.8	51	24	38.8	55	47	51.1	30.65	28.85	30.05	67	50	61.7	0.493
February	57	25	41.8	57	25	42.4	59	48	53.3	30.50	28.98	29.78	68	49	59.5	1.109
March	55	25	39.2	53	26	39.1	54	44	49.9	30.60	29.29	29.94	64	45	56.5	0.195
April	77	31	48.1	77	31	48.3	67	47	56.7	30.31	29.30	29.92	64	42	54.0	0.445
May	84	44	57.9	83	44	58.6	68	58	62.3	30.41	29.03	29.82	73	41	54.2	2.875
June	77	48	60.3	76	50	61.5	68	58	61.8	30.38	29.64	30.02	72	37	51.2	1.552
July	85	48	66.5	85	54	67.5	74	63	68.4	30.28	29.60	29.93	80	44	60.1	0.335
August	80	52	66.7	80	53	67.2	73	64	68.3	30.24	29.55	29.86	70	45	55.8	1.764
September	72	38	55.5	72	39	55.7	66	57	60.9	30.22	29.35	29.82	70	46	56.0	1.940
October	68	39	55.2	67	39	55.6	64	56	61.2	30.20	29.22	29.88	73	50	61.9	0.767
November	56	28	40.8	56	28	40.6	58	48	53.2	30.08	28.68	29.53	75	53	63.1	2.455
December	49	23	37.9	49	23	38.7	54	46	50.6	30.41	29.37	29.93	78	55	65.5	0.476
Whole year	"	"	50.7	"	"	51.2	"	"	51.8	"	"	29.86	"	"	57.4	14.206

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

The hygrometer was re-adjusted before the observations for this year were begun, it indicated too much moisture by nearly 30 degrees.

Variation of the Magnetic Needle, 1807.
September - 24° 10′,2. West.